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F. xvii. m 63

A LETTER

TO

JOHN HAYGARTH, M.D. F.R.S.

FROM

COLIN CHISHOLM, M.D. F.R.S.



G. Woodfall, Printer,
Paternoster-row.

A
LETTER

TO

JOHN HAYGARTH, M. D. F. R. S.

LONDON AND EDINBURGH, &c.

FROM

COLIN CHISHOLM, M. D. F. R. S. &c.

AUTHOR OF

AN ESSAY ON THE PESTILENTIAL FEVER:

EXHIBITING

FARTHER EVIDENCE OF THE INFECTIOUS NATURE OF THIS
FATAL DISTEMPER IN GRANADA,

DURING 1793, 4, 5, AND 6;

AND IN THE UNITED STATES OF AMERICA,

FROM 1793 TO 1805:

IN ORDER

TO CORRECT THE PERNICIOUS DOCTRINE

PROMULGATED BY

DR. EDWARD MILLER,

AND

OTHER AMERICAN PHYSICIANS,

RELATIVE TO

THIS DESTRUCTIVE PESTILENCE.

Sensus; quod nisi prima fides fundata valebit,
Haud erit, obcultis de rebus quo referentes
Confirmare animos quidquam ratione queamus.

LUCRETIVS *de Nat. Rerum, lib. 1.*

LONDON:

PRINTED FOR JOSEPH MAWMAN, IN THE POULTRY.

1809.

A LETTER, &c.

MY DEAR SIR,

Clifton, March 31, 1808.

WE are, I am confident, both engaged in the same cause, the cause of truth and humanity; and I feel satisfied that whenever or wherever that cause is unjustly attacked, we shall be equally ready to vindicate and defend it. The frequent opportunities you have indulged me in of a free and reciprocal communication of sentiment on medical subjects, and more especially that most important one, infection, and the propagation of certain descriptions of fever by contagion, leave me no room to doubt that we think alike on it.

It is, no doubt, in your recollection how often we have regretted the diversity of

opinion which unhappily prevails in North America, on this interesting subject. Some of the physicians of that country, we have seen and lamented, entertain a very different opinion from ours, of the fever which has prevailed with so much destructive violence at different times in their cities; and have assigned a cause, which, in our unrestrained discussion, we could perceive no rational, no existing grounds for. It is also doubtless in your recollection, that one of these physicians, Dr. Miller, of New York, has published a “ Report on the Malignant Disease, which prevailed in the city of New York, in the autumn of 1805, addressed to the Governor of the state of New York,” calculated for the maintenance and establishment of the opinion he and a few other American physicians have promulgated, viz: that this “ Malignant Disease” has owed its origin to local and domestic causes alone. And you will likewise remember, that to promote the impression his “ Report” may have made on the public mind, towards the establishment of this opinion, Dr. Miller, has, in an appendix to it, taken occasion to

animadvert on the essay I some years ago published on the malignant pestilential fever of Grenada, with a view to impugn the opinions I have therein upheld, and to subvert them by endeavouring to prove that these opinions were peculiar to myself, that the malignant pestilential and yellow remittent fevers of the West Indies, are precisely the same disease; that my deduction of the former from the pestilential state of the ship Hankey, is founded on erroneous principles, false information, or fallacious appearances, and that in a letter of mine written to a friend, published without my concurrence, I have virtually relinquished the doctrine and principles I maintained in my essay. This very extraordinary report, and the still more extraordinary animadversion intended to support it, is the immediate occasion which presents me with the pleasure of addressing you. Considering, however, the statements I published in my essay, as founded on principles which were generally admitted and incontrovertible; it was my fixed resolution to maintain silence in every instance

of controversy or opposition. Now I fear silence might be forced into the construction of conscious inability, and consequent abandonment of opinion. And I know it will be a source of real gratification to you, and every other physician, who, like you, has exerted himself in the cause of humanity, with an assiduity, an energy, a benevolence, and an acuteness and accuracy of observation worthy of such a cause, to have my doctrine and opinions vindicated from the insidious ratiocination of a speculatist, and to see them remain uninjured by the open and avowed promulgation of a theory subversive in its application, of the means of public safety from infection and pestilence.

In the execution of this intention, to which it is my purpose to devote this letter, I shall offer to your consideration, and through you to that of the public, some remarks tending to demonstrate that my opinions concerning the malignant pestilential fever, as it appeared at Grenada, and the other West India islands, in 1793,

4, 5 and 6, are not “singular;” that this fever and the yellow remittent are not “precisely the same disease;” that my deduction of the disease, from the pestilential state of the Hankey is just, correct, and supported by evidence corroborative of that which I received from Mr. Paiba, and that in my letter published in a mutilated form in the medical repository of New York, and quoted by Dr. Miller, I have not relinquished the doctrine and opinions I upheld in my essay. To this I shall add some observations on Dr. Miller’s report itself. I wish it to be understood that in doing this, I mean to apply my observations and remarks to opinions not men, and as I have said in another place, and on a similar occasion, although Dr. Miller, and the other medical gentlemen of the United States of America, who hold similar opinions, have doubtless long since been confirmed in the truth of the proverb, “obsequium amicos, veritas odium parit,” yet would I reluctantly hazard the latter, by offering my sentiments on the doctrine laid down by them, did I not

consider that all enlightened minds are open to the impression of truth, and that in scientific pursuits, it is not the man but the opinion which is the subject of disquisition." (Essay, V. ii. 260.) I am aware and I regret that this is not precisely their way of thinking; I am aware that some of the most respectable of these gentlemen, hold in sovereign contempt the opinions and the sentiments of medical men of Europe, but more especially of Great Britain, when they militate against, or have a tendency to contravene those they have thought proper to entertain and impress; and that they more especially exclaim against their interference, their well-meant and benevolent interference, in endeavouring to correct their misstatements, their misconceptions, and the consequent and frequently injurious impressions which their speculative notions may have made on the public mind. I am aware that medical anathemas are hurled at our heads for doubting that they, like their prototype in arrogated supremacy, the Roman Pontiff, are the arbiters of the fates of men. Astonishing that men of excel-

lent talent, learning and ingenuity, should thus claim exclusive knowledge, should thus “proclaim that they will exercise their senses, their understandings, and their judgments, free from all authority, dogmatism, or any other controul, save that of truth alone,” without reciprocal concession; when they must be conscious that European and West Indian physicians are fully as much entitled to proclaim and act upon similar sentiments! when they must advert to the principles which they themselves lay down, that they have an equal right to judge of their local circumstances, and of the degree of influence they possess in the production of pestilential diseases! This right, you know they have dictatorially deprived me of; my essay with them is a mere “book of positive declarations,” consequently, they would imply, a book of no authority, and yet they have never seen the country wherein the malignant pestilential fever, the subject of that book, originated, and from which it spread. This is popish arrogance with a witness! When I impugn their doctrine of locality of cause, I am conscious I have *some right* to do so,

several years' residence in or near Philadelphia and New York, during the revolutionary war, having presented me with adequate opportunities of knowing the local circumstances of these cities as they relate to the production of disease. But these anathemas, this arrogated supremacy I regard not. The feelings of conscious rectitude, and the completest conviction of the justice and truth of my cause, will urge me to censure where censure is due, to applaud where applause is merited.

We should imagine that the strong facts published by the boards of health of Philadelphia and New York, by the college of physicians of the former, and by several individual physicians of the first respectability in the United States, would have been sufficient to give a right direction to the minds of the people of North America, relative to the cause of the epidemics, under which they have so often suffered, and to the true means of preventing their recurrence. One would conceive that the most sceptic unbeliever of infection and its

consequent evils, would yield to these plain and simple details of its introduction and diffusion, that the “*turba inanium verborum*” of “speculative chemists, and declaiming physicians,” would ere now be cleared away by the light of reason and the lessons of experience; in a word, we should expect that opposition would cease here, that “singular opinions” would give place to those formed on the experience of almost every enlightened *resident* physician in the West India islands; to opinions not coalescing through design, but held by men distant from each other, without connection, even without knowledge of mutual sentiments. You will recollect that Dr. Miller’s report is not the first attack I have sustained from the editors of the Medical Repository. The late D. E. H. Smith exhibited even a greater degree of intemperance than Dr. Miller, in his “Account of the origin of the pestilential fever at Grenada.” *Med. Rep.* V. i. 471. But I had abundant reason to hope that these gentlemen had been impressed with a clear and decided conviction, not only

of the purity of my intentions in first publishing on the malignant pestilential fever; but of the correctness of my statements in delineating its history and its features. I had also ample reason for believing that my accuracy respecting the melancholy account of the fever on board the Hankey, at Boulam, had been fully established in their minds. A reference to the introductory declaration of Drs. Mitchell and Miller, will shew I apprehend, that my belief was well founded. *Med. Rep.* V. ii. 285. They say that I indignantly repelled Dr. Smith's censure on my conduct, and in doing so I manifested the warmth of conscious integrity, and the irritation of offended honor. Notwithstanding this ample concession on the part of the editors in general, and this apology for the scandalous obloquy of Dr. Smith, I see myself again exposed to the petulant sarcasms and absurd hypercriticisms of one of these very gentlemen, who, at a former period, apologized thus amply for the impertinence of his colleague.

Two propositions which must be admitted, overthrow the whole of the fabric so industriously raised by Dr. Miller. The first is, that the facts stated by me respecting the importation of infection, and the subsequent diffusion of disease by contagion, are almost universally admitted by *resident* physicians in the West India islands, and by a great many of the more enlightened and liberal of the North American states: the second is, that Drs. Mitchell, Miller, and Smith, have, in every instance, confounded the malignant peti-
 lential (improperly named yellow) and the yellow remittent fever. Before I present you, however, with evidences of the validity of my first proposition, I shall take occasion to request the editors of the Medical Repository to explain the sense in which their readers are to receive the following animadversion on my doctrine, as published in the last edition of my essay. “ Dr. Chisholm,” say they, “ by no means contends that the yellow fever of the West Indies, *arises from contagion*; but his opinion seems to be, that contagion ge-

nerated on board the Hankey, in 1793, *spread by contagion* over many of the islands, and much of the continent of North America, exciting a specific form of disease not known in any of those regions before." Med. Rep. V. v. 228. If this is not an instance of that involution of opinion and conception I have mentioned, I know not what is; it is more charitable to assign it to declamatory petulance, than to a predetermined design to bewilder the minds of the less enlightened public. Four years prior to the publication of the 5th Vol. of the Repository, the following delineation of my sentiments on the subject of the foregoing very singular passage, was admitted into the 2nd volume, with the most marked approbation. I have there said, that "any person who attentively and candidly peruses the passages of the essay which relate to this subject, and who sees things in their true light and form, must observe that I have no where mentioned, far less maintained, that the plague prevailed on board the Hankey; they will perceive, that a fever

proceeding originally perhaps from the inclemency of the season, and the circumstances of the situation of the adventurers, had become by confinement, filth, consequent impurity of air, and depression of spirits, a true jail fever, or a fever of infection, heightened to an almost pestilential violence. Infection proceeding from a source such as this, and retained and concentrated by the preservation, the pertinacious preservation of the bedding and clothes of the dead, might certainly have been sufficient to produce the most dreadful effects." *Med. Rep.* V. ii. 290. Where then is the discrepancy they mean to insinuate. An uniformity of doctrine and opinion prevails in my publications of 1795, 1798, 1800, and 1801, and of their truth I feel the most perfect conviction at this moment; my experience has only served to strengthen them. Yet after all this, Dr. Miller comes forward in seeming oblivion of preceding statements, and torturing new but similar circumstances of infection, which had since occurred in *Demerary*, into violent coincidence with his

own theory, he thus, six years after tauntingly exults in this conclusion, "If Dr. Chisholm's story be admitted, it is only an instance of malignant disease, generated in a vessel, *as he does not pretend to derive it from the Africans.*" Elicit any useful meaning from this, if you can! Again, "As to Dr. Chisholm's opinion of the contagiousness of these fevers, it rests upon the same vague and delusive foundation with the popular, or rather vulgar inference of contagion; in all cases where a disease attacks a great number of persons in the same vicinity." If this contains any meaning, it is, that we are to resist the demonstrations of our senses, and yield our understandings to the dogmas of a theorist. Dr. Miller will do well to recollect, that "*Interdum vulgus rectum videt,*" and not to discover that plasticity of faith which assents or dissents from the *vulgar opinion*, just as it suits the tendency of his own or of those connected with him. Mark the exhibition of this, in the assignment of pestilence to eclipses, comets, volcanic eruptions, earthquakes, and the other convulsive

efforts of nature to restore the equilibrium of her system, by one of the principal sticklers to the theory of Dr. Miller. Phenomena, which in less enlightened ages, struck terror into the vulgar, and were viewed by the superstitious as the harbingers of the greatest evils incident to mankind! These modern theorists spurning the idea of infection and contagion as a vulgar opinion, adopt that which is still more vulgar, and which existed only when the laws of the economy of nature were hid in darkness, and when the human mind rose in intellect little above brutal instinct. Notwithstanding the ingenuity, industry, and learning, manifested in Mr. Webster's volumes on pestilential diseases, and the relation which he supposes to exist between them and the various "phenomena of the physical world," I suspect the true philosophical enquirer will consider the seeming coincidences he states, as purely accidental, and that none but those whose minds are in the habit of a peculiar way of thinking, can discover the dependance of cause and effect in them. How can we

lean to the theory of a man who can uphold an opinion which throws all nature into its original state of chaos, who indeed, seems to believe in a *τὸ θεῖον*, and yet considers its operations as fortuitous and equivocal.

Experience is undoubtedly the standard of our belief in all matters which have relation to physical doctrines. If they are disproved by it, we reject them; if they are again and again demonstrated to be true by experience, we cherish them. In this respect, experience stands in direct opposition to prejudice. Now if it is asked what is meant by experience, I answer in the following manner: if an object is presented to my senses, I endeavour to assure myself whether the sensations produced by it are clear, definite, distinct; whether they are such as I cannot hesitate to consider genuine and natural. If the same object is again presented to my senses, in the same circumstances, and if again the same sensations are produced by it, I shall have every reason to be confirmed in the

conclusion I formerly made; and I shall have every inducement to act upon it. The reiteration of the same impression of the same object in the same circumstances on my senses, is therefore experience, or the criterion by which we ought to form our opinions, and regulate our conduct in physical discussions. My meaning will be more perfectly conceived by adopting the induction of a more skilful reasoner. "If I wanted to certify myself concerning any particular sense or percipient faculty, that it is neither depraved nor defective, I should attend to the feelings or sensations communicated by it, and observe whether they be clear and definite, and such as I am, of my own accord, disposed to confide in without hesitation, as true, genuine, and natural. If they are such, I should certainly act upon them till I had some positive reason to think them fallacious. Secondly, I consider whether the sensations received by this faculty be uniformly similar in similar circumstances; if they are not, I should suspect either that it is now depraved or was formerly so; and if I had

no other criterion to direct me, should be much at a loss to know whether I ought to trust the former or the latter experience; perhaps I should distrust both. If they are uniform, if my present and my past experience do exactly coincide, I shall then be disposed to think them both right. Thirdly, I consider whether, in acting upon the supposition that the faculty in question is well informed, I have ever been misled to my hurt or inconvenience; if not, then I have good reason to think that I was not mistaken when I formed that supposition, and that this faculty is really what I supposed it to be. Fourthly, if the sensations communicated by this faculty be incompatible with one another, or irreconcilable to the perceptions of my other faculties, I should suspect a depravation of the former: for the laws of nature, as far as my experience goes, are consistent, and I have a natural tendency to believe that they are uniformly so. It is therefore a presumption, that my faculties are well informed, when the perceptions of one are quite consistent with those of the rest, and with one another. In a state of solitude I must sa-

tisfy myself with these *criteria*; but in society I have access to another criterion, which, in many cases, will be reckoned more decisive than any of these, and which, in concurrence with these, will be sufficient to banish doubt from every rational mind. I compare my sensations and notions with those of other men, and if I find a perfect coincidence, I shall then be satisfied that my sensations are according to the law of human nature; and therefore right." (Beattie's essay on truth, p. 2. c. 1. s. 2.) If this reasoning be applied to the subject before us, it will, I apprehend, be a perception of common sense, that the importation of infection gave origin, and that the emanation of contagion gave wide diffusion to the malignant pestilential fever. If I have repeatedly seen, or known from undoubted authority, that healthy persons visiting a certain ship, soon after became affected with a certain train of morbid symptoms, which in a few days or hours deprived them of life, I have every reason to be satisfied that these persons received their illness and death from some

deleterious principle peculiar to the ship they visited. If, again, I have repeatedly seen, or known from undoubted authority, that other healthy persons approaching to within a certain distance from those diseased persons, or coming in contact with them, have soon after become affected by the same train of morbid symptoms, I am fully justified in being assured that a deleterious principle of the same nature has been received from the bodies of the last into those of the first, producing the same effect. If again, a ship or other vessel, on board of which any of these persons thus affected are or have been in a diseased state, or whose clothes have been preserved on board, imbued with the effluvia proceeding from their bodies in a diseased state, proceeds to another country; and if the healthy of that country, on visiting or having communication with this ship, become affected with the same train of symptoms I have observed to take place in the first instance, and if from them other healthy persons are similarly affected, I am fully supported when I express my firm convic-

tion, that this second disaster has been produced by the introduction of the deleterious principle proceeding from the first vessel or ship. Further, if nothing locally, or previously existing in the first or second country was ever seen or known by myself, or seen or known by others of undoubted authority, to have produced such a train of morbid symptoms as was observed to arise from the operation of the principle imported into either country in the manner stated; I possess the strongest proof that nothing local and previously existing in these countries, could have been the cause of this train of morbid symptoms. In innumerable instances, my own senses have borne testimony to this; but although perfect conviction is the consequence in my own mind, yet the same conviction may not result in the minds of others from this testimony. I proceed, therefore, to show that there is undoubted authority for it in the testimony of others. Let me only further observe, that “on the first publication of any extraordinary account, or even of any article of ordinary intelligence, no one, who

is not personally acquainted with the transaction, can know whether it is true or false, because any man can publish any story. It is in the future confirmation or contradiction of the account; in its permanency or its disappearance; its dying away in silence, or its increasing in notoriety; its being followed up by subsequent accounts, and being repeated in different and independent accounts, that solid truth is distinguished from fugitive lies." On this position of Dr. Paley's, I am willing to rest the validity of my story. Are the editors of the Medical Repository, and the other endemists of North America, equally willing to submit their opinions to the test of this reasoning, to rest their story on this position?

PROPOSITION I.

That my opinions concerning the malignant pestilential fever, as it appeared at Grenada, and the other West India islands in 1793, 4, 5, and 6, are not singular.

1. I am satisfied in my own mind, that what I have advanced in my essay is sufficient to convince the rational and the unprejudiced, and I should consider a further detail of facts as superfluous, did not the unwarrantable assertions of Dr. Miller, I have already directed your attention to, seem to require it. My respectable friend Dr. John Stuart, at the request of Dr. Hossack, and the mayor of New York, drew up an account of the malignant pestilential fever of Grenada, in the form of a letter addressed to the former of these gentlemen, and dated November 12, 1805. It cannot be supposed that he was under any influence but conviction of the truth, and the truth he states thus: "As

to the character of this fever, my experience has fully satisfied me that it *was specifically distinct from every form of the indigenous bilious remittent, which I had ever observed.* Because it appeared at a season of the year which I had always found healthy during a period of nineteen years I had resided in the colony; because it did not appear particularly in those situations where bilious remittent fever usually prevailed, during the unhealthy season of the year; because there was an evident difference in the character and type of the two diseases," (which he here describes minutely) "because I never knew this fever terminate within a few weeks in intermittent, as tertian, remittent or bilious fever commonly does; because I did not find the same mode of treatment successful in both cases of fever; for the early, and bold and free use of bark, which I have found very generally to answer in bilious fever, seemed to aggravate this fever, and to hasten the fatal issue." The Doctor then states his reasons for believing the fever contagious, which, as they correspond exactly with those I

have given in my essay, I forbear to quote. The following however, as it decidedly disproves Dr. Miller's assertion of my opinions being singular, cannot be passed over. " From a thorough belief in the minds of all the medical gentlemen in Grenada, who witnessed the disease, that it was so; let it be observed, however, that one of the most respectable physicians in St. George's, and a particular acquaintance of my own, would not allow *at first*, that it was contagious. And lastly, from a full conviction that I, as well as some other medical gentlemen, contracted the disease in our attendance on the sick. Respecting the propagation of this fever, I *am decidedly of opinion*, that it was occasioned by visiting infected apartments, or by the near approach to, or contact with people labouring under it. There is every probability also, that the infection was brought to Grenada by the Hankey; but what its nature was, whether it originated on board in consequence of the number crowded together while labouring under the endemic of a warm climate, and that in a sultry moist

atmosphere, is a question I do not take upon myself to answer." New York, Evening Post, November 26, 1805; and Additional Facts, &c. of the college of physicians, Philadelphia, 1806. p. 77.

2. It was our friend Dr. John Gordon's intention, you know, to publish the very valuable observations on the malignant pestilential fever, which had occurred to him at St. Croix, where his opportunities as royal provincial physician, for the Danish American islands, were extensive. The substance of these observations he communicated to you in a letter, written I believe at your request; but the publication of them has been most unhappily prevented by death, which has deprived society of one of its most valuable members. As these observations elucidate and support my sentiments, I feel particularly gratified in being able, through your kind attention, to present the whole of this letter to the public. They will there find irrefragable proofs of the truth of my proposition, that the cause of the malignant pestilential fever of Gre-

nada, and the other West India islands, of 1793, and the subsequent years, was imported infection. As any abstract, however perfectly executed, might tend to weaken the salutary impressions which so clear and obvious a detail must certainly make on the public mind, I shall not attempt it, but consider its importance as a sufficient apology for giving it as it came from the pen of the judicious and upright writer himself. I shall only in general observe that Dr. Gordon's observations embrace every important object connected with our subject; the importation of infection, its diffusion, and their causes—ignorance, perversity, selfishness, or the abstraction of the sentiment of public good from the consideration of the consequences of imported infection; the abuse of all preventive measures, in the promotion of “the speculations of cupidity, and the calculations of venal men,” and, in a word, the elusion of the laws of quarantine, in the prostration of truth and humanity. All this is most clearly proved; and all this, we are morally certain, is applicable to the epidemics of the

United States of America, since the year 1793, and in a more particular manner to those of New York and Philadelphia. This letter, the reader will perceive, is a very valuable document in the history of the malignant pestilential fever, from the intrinsic importance of the facts it relates, from the lessons it holds out to commercial countries, and from the real worth of its writer, who you know as well as I do, was actuated by no impulse but the love of truth and humanity. You will perceive a material difference from one part of the account I have given, Essay, Vol. 2. p. 342, in as much as Dr. Gordon, in his letter positively says, that the infection spread among the inhabitants, on that occasion, but was chiefly fatal to new-comers. See Appendix, No. 1.

3. Dr. Dancer, of Jamaica, with whom I have not had the smallest acquaintance, has, you perceive, very handsomely vindicated my character from the aspersions which have been thrown on it. This gentleman having in a very clear, distinct, and impartial manner

stated and examined the various opinions on this subject, and the various arguments advanced to support each respectively, he deduces from the whole that the "yellow fever" is an imported disease, and is communicable by contagion. "Having thus stated," says he, "as briefly and explicitly as possible, my sentiments on the subject, I shall conclude by making references to the facts and circumstances on which my opinion is founded. In the first place I must refer to the facts stated by Dr. Chisholm, which, notwithstanding their being called in question, *have not yet been invalidated*, (Sept. 1803) *and remain, in my judgment, unanswerable*. Secondly, Although Dr. Chisholm has been represented as being almost the only advocate for contagion in the West Indies, *the contrary of this is the truth. Most of the practitioners in the West Indies, concur with him, and various facts and cases have been brought in proof of what he has stated*. Many similar instances of the contagious nature of yellow fever are related in the Medical Repository, which ought to have at least as

much weight on the one hand as any stated to the contrary should have on the other. It is well known to the practitioners of this island, that the pestilential fever prevails only on certain occasions; that it generally begins among seamen and soldiers lately arrived from Europe; that it has no apparent connection with local causes, that is to say, *it makes its appearance during the most healthy seasons, and in the healthiest situations; while, on the other hand, it is never found to prevail in unhealthy or marshy spots.* Lastly, it appears contrary to what has been again and again asserted, to be directly communicable from one person to another, and from a single person to many: for example, one man only, going on board an infected ship, and contracting the disease, shall communicate it to several others, who never had been otherwise exposed to the infection," which Dr. Rush has admitted, (Med. Rep. V. 6. p. 160.), and cannot be explained upon his principles. How can he extricate himself from his own embarrassing statement. "The odour," says he, "emitted

by persons in the confluent small-pox, has been known to produce the same symptoms, together with a subsequent fever and aphthous sore throat. This has been remarked long ago by Dr. Lind, and latterly by Dr. Willan, in his Reports of the diseases of London. That the yellow fever is often excited in this way, *without the intervention of a supposed specific contagion, I infer from its sometimes spreading through whole families, who have breathed the same impure atmosphere with the person first affected by the fever. This is more especially the case where the impression made by the exhalation from the sick person is assisted by fear, fatigue, or anxiety of mind in other branches of the family.*" What is this but the full admission of contagion? The fact I believe is, that this excellent and estimable physician, as well as the gentlemen of New York, have bewildered themselves in the mazes of their new-fangled theory of infection and contagion; their endeavours to establish "the difference between secreted contagions, and morbid excretions from the body;" between

the “ vitiated product of living vascular action,” and “ the venomous offspring of putrefaction,” and thus losing sight of common sense, the best guide in physical as in moral disquisitions. Dr. Dancer proceeds to give a remarkable illustration of his opinion, from Mr. Brown, of the 60th regiment, stationed at one of the most healthy situations in the West Indies, Up-park, in Jamaica, and finishes by observing “ the conclusion is not to be withstood.” Let us compare the foregoing candid and most evidently impartial statement, with one or two of *the very liberal* remarks of Dr. Miller, in the appendix to his “ Report.” “ Without recurring,” says he, “ to facts of this kind, Dr. Chisholm’s doctrine, considered in itself, cannot stand the test of examination.” An assertion without a proof! but what are the facts? Why mere fabrications of Dr. Miller; thus “ the great body of physicians and people in the West Indies, do not find the fever *now prevailing* (i. e. in 1806; observe the artful evasion! In my essay the subject of discussion is the fever; the ma-

lignant pestilential fever of 1793, 4, 5; and 6), at all different from what it was many years before the arrival of the ship Hankey, from Boulam." To animadvert on this "equivoque" is unnecessary; the discerning reader will readily see through the artifice.

4. The result of a calm, dispassionate review of the writers on "yellow fever," (malig. pest. fever), and of its progress from its centre, at Grenada, to the wide limits of its diffusion, is thus given by Mr. Tytler; "this account of the origin of the fever at Philadelphia, as we have already seen, is inadmissible by those who deny the contagious nature of the disease; but as the latter have never given any distinct account of its rise, or shown why it should first appear in one island and then in another, instead of beginning in them all at once; we must adhere to that of Dr. Chisholm, till we are furnished with a better." He again remarks, what must indeed be obvious to every observer, "that if after such repeated and dreadful expe-

rience of the bad effects of allowing putrid matters to accumulate, such quantities could be collected as to produce" this very fatal sickness, " it argues a most unaccountable and indeed incredible insensibility on the part of the people, as well as remissness on that of the magistrates; *and this perhaps may be accounted as strong an argument in favour of contagion as can be adduced.*" Treatise on yellow fever, p. 491, 494. Although seven years have elapsed from the period at which this remark was made, till 1805, yet in that year, the physicians of New York have attributed the recurrence of the same most destructive disease to the same causes. They themselves have promulgated the sentence of condemnation which such conduct merits. " A poison," say they, " which in the city of New York, has destroyed, within three months, the lives of more than twenty practitioners of medicine, well deserves to be traced and understood by the survivors. And yet with all these opportunities to acquire knowledge on the subject, and with all these incentives to vigilance and care,

the American physicians and philosophers, *who have viewed the rise and progress of pestilence, walked amidst it by day and by night, year after year, beheld their friends and fellow-citizens falling around them as on the field of battle, endured its violence in their own persons almost to the extinction of their lives, and suffered from it largely in the depreciation of their property; these men we say, are considered as incompetent witnesses before the judges of the medical tribunals of Europe.*" Med. Rep. V. 5, p. 182. How does declamation betray their imprudence! they acknowledge and describe all the effects, the direful effects of contagion, and yet deny its existence; an effect without a cause. Incompetent witnesses! What impartial judge, what honest jury can trust a testimony the basis of which is prevarication and absurdity. But it is to be lamented that as long as preconceived theories, as long as mere chemical speculations are applied to the investigation of the causes of wide spreading diseases; as long as obvious facts and opinions, established thereupon, are

superseded by the perceptions of fancy, and the dogmas of overweening conceit; as long as *abstract* are substituted for *aggregate* conceptions in pathological disquisitions, so long must and ought Dr. Miller and his party, to be considered as “incompetent witnesses before the judges of the medical tribunals of Europe,” or of any country in whom the despotism of prejudice holds not imperial sway; so long must and ought these gentlemen to be considered as “incompetent witnesses,” before the tribunal of truth and humanity. This poison most certainly well deserves to be traced and understood by these survivors; but unavailing must their efforts be to trace and understand it, whilst their minds are confined by the shackles of the monster septon, whilst they neglect what ought to be the object of their investigation, and descant on what has no real existence, or what is irrelevant to the production of the evils under which they and their fellow citizens unhappily suffer. You will foresee that I shall be classed “among the most rigid and intolerant” of these judges; but whilst

I thus assume this character, influenced by the feelings of conscious integrity, I declare to you, it is with the utmost reluctance; for I do not hesitate to avow that I cherish a very high and sincere respect for the abilities and character of some of the gentlemen, whom I ever must oppose in this one most important point, and I should regret exceedingly that diversity of opinion should have attached to it any thing like personal animosity, of which indeed there is no impression on my mind.*

5. A remarkable circumstance is here deserving of being particularly stated, not only as it furnishes an additional evidence of the fact of imported infection into the

* I request Dr. Rush, and Dr. Mitchell will accept the advocacy of Tristram Shandy, in extenuation of my warmth. "There is one instance, which I own puts me off my guard; and that is when I see one born for great actions, and what is still more for his honour, whose nature ever inclined him to good ones; when I behold such a one, mounted though it is but *for a minute beyond the time which my love to my country has prescribed for him*, then I cease to be a philosopher, and for the transport of an honest impatience I wish *the hobby horse*, with all his fraternity at the devil." TRIST. SHANDY, V. i. c. 8.

West India islands, but as it proves the correctness of my statements. You will see, by turning to the second volume of my essay, p. 314, that I have stated the origin of the malignant pestilential fever at Tortola. Please to remark the coincidence between the passage I have referred to, and the following letter I have just received from Dr. M'Gregor; if any thing can tend to the establishment of truth, it must be this undesigned unpremeditated correspondence of observation of the same fact.

SIR,

Portsmouth, 3rd Jan. 1808.

Without any communication of views or of opinions, I find that I have been concurring with you in a very important and much disputed point, viz. the contagious nature of fever in the West Indies. I myself entertain no doubt that the fever which I saw in Grenada, in the year 1796, and which proved so destructive to the army there, was communicated by contagion. I understand contagion in the sense in which Dr. Adams explains it in the second edi-

tion of his work on morbid poisons. The strong circumstances which led me to conclude that the fever which I saw at Grenada and Tortola was contagious, I have detailed in the appendix to the Sketches of the expedition from India to Egypt. A very recent reference to my notes and case books, written in the West Indies, confirms me in the opinion which I had formed of this fever. I have likewise lately corresponded with my friend Mr. Bruce, Surgeon, 88th regiment, on this subject. Mr. Bruce is well qualified to speak on the subject, he was my assistant in the 88th regiment, and was with me in the Betsy transport, at Grenada and Tortola. In his letter, Mr. Bruce offers some remarks and corrections of the account of the fever, which I have appended to the Sketches. He says, that "the fever which prevailed among the 88th regiment, in the Betsy transport, in the West Indies, in the year 1796, was manifestly contagious and very fatal. A large proportion of the officers of other regiments, particularly of the 8th, were ill of it at the same time," &c. By a singular

coincidence which I think forms the strongest corroboration of your statement, without the most distant knowledge of me or of my opinions, occurs in p. 314, of the second volume, second edition of your work, in which you allude to the melancholy situation which Mr. Bruce and myself witnessed at Tortola, and of which I have given some account in the appendix to my Sketches,

I am, &c.

JAMES M·GREGOR.

6. Dr. Gilbert Blane, you know, about two years ago, drew up some observations on this subject for the information of the Prussian government, which are highly important, and which tend very materially to support the views I have taken of it. It is true he seems to have found cause, from his extensive experience during four very active naval campaigns in the West Indies, to extend the recurrence of the infection of "yellow fever" to those years in which these campaigns took place, and consequently much beyond the number of in-

stances I had been aware of: but the reiteration of the same cause in the same circumstances, producing the same effect; and the observation of that reiteration, the result of the experience of an acute, a skilful and an honest physician, establishes more firmly the positions I had taken on more limited grounds. It may be objected to me that Dr. Blane's silence relative to the introduction of infection into Grenada, and thence into the other islands and North America in 1793, constitutes something like conviction in that gentleman's mind, that the whole is a fable, a fabricated story. But besides the internal evidence against an inference of this kind, and besides the manifold proofs I have adduced in my essay and in this letter, I have Dr. Blane's assurance that no such inference can be admitted. "I can no otherwise account," says he, in a letter he has favored me with, dated 2nd January, 1808, "for my not quoting your book, than that it really did not occur to me, and you will take along with you that in the course of four very active and interesting campaigns in the West Indies, I

was led to the persuasion of the infectious nature of that fever." This very circumstance indeed, this non-occurrence of my book to the recollection of Dr. Blane, at the time he drew up his observations for the Baron Jacobi, is a satisfactory proof of the truth of its doctrine; since we see, that in the first instance, long prior to the event which gave origin to it, and in the second, without reference to it, ten years after that event, Dr. Blane had observed, almost precisely as I have done, and has cherished the same doctrine as the necessary result. The only circumstance in which we disagree is respecting the origin of the "yellow fever" at Philadelphia, in 1793, he assigning it to the influx of French emigrants into that city in July of that year, I to the importation of infection from Grenada, or some other of the islands infected from it. This however is of little importance, seeing we fundamentally agree. Perhaps indeed Dr. Blane may have assumed the opinion from the general principle, that a crowded population suffering under depression of spirits, and privation of every ex-

ternal comfort, may give origin to infection; which in combination with a tropical temperature of atmosphere, may generate the monstrous production, which we have found to be the effect of such a state of things. I shall not enter here into any controversy on this point; I may however observe that I am more inclined to be confirmed in this opinion, from Dr. Blane's not objecting to it, when I submitted this passage to his inspection. Had he founded his assumption on particular specified facts, or on any other ground but rumour, no doubt he would have said so. I am convinced, and most of the West Indian and North American physicians are so, that the same virus which gave birth to the malignant pestilential fever of Grenada, was also the parent of that of Philadelphia of 1793, by means of the trading vessels continually passing between these places. See Essay, V. i. p. 218, 238,

7. I might here, with very great propriety, indeed the nature of my subject may be said to require it, adduce as a

further, and perhaps a stronger corroborative evidence of the truth of my proposition, the importation of infection into some of the maritime cities of Spain, and thence into the fortress of Gibraltar: for the calamities these places suffered were the genuine offspring of the infection introduced by the Hankey into Grenada, and thence into Philadelphia, in the year 1793. The catenation of proof is evident and distinct, and supported by a number of concurring circumstances. I might state the infatuation, or the obstinate and wilful resistance to the truth of obvious facts, and to the deductions from the demonstrations of sense confirmed by the dictates of reason, and the inferences of experience, of the superintendants; and the fatal delusion, or servile and culpable submission, of the subordinate medical officers of Gibraltar, of that period. I might indulge in the pleasing and gratifying eulogy of the subsequent unbiassed and judicious investigation and development of cause, the discovery and delineation of truth, contrasted with the immediately preceding confusion or maze of error

or dogmatism, dictating to ignorance or base servility. In fine I might fortify my own conclusions by the statement of the clearest evidence of imported infection of Dr. Fellowes, who so ably and honourably fulfilled the salutary measures too tardily adopted by his employers. All this I am possessed of the means of doing, but I decline this very important duty to the public, as there is, I understand, a prospect of its being more ably and more completely performed, by Dr. Fellowes himself, or by Major Wright of the royal artillery, whose opportunities during the whole of the fatal epidemic, and whose talent for observation, and humane assiduity and enlightened mind, have so well fitted him for it. All I feel myself permitted to do, I have done. I have introduced the letter you have favored me with on this interesting subject, and I have added some important general circumstances communicated by a learned and skilful physician, who arrived at Gibraltar almost immediately after the ceasing of the pestilence. See Appendix, No. 2.

8. I might also in this place dwell on the fatal consequences which resulted from nearly similar dogmatism or error, exhibited by the medical staff of the St. Domingo army, at the Cove of Cork, in the year 1795, 6; more especially as the sufferings of the British troops stationed at Cape St. Nicola Mole, in St. Domingo, are clearly attributable to it. As I have so amply stated the result of my enquiries and observations concerning this very fatal fever in my Essay, Vol. i. p. 203, 208. I shall not enlarge on it here, further than to refer you again to the judicious remarks of Dr. Gordon on the writings of those physicians who have described the sufferings of the garrison of Cape St. Nicola Mole, which may probably find a place in the Medical Journal of Edinburgh.

These important facts I trust you will find sufficient to establish my first proposition, viz: that the importation of infection and the subsequent diffusion of the malignant pestilential fever by contagion are almost universally admitted by resident

physicians in the West India islands, and by a great many of the more enlightened and liberal of the North American States. Some facts and observations which I shall submit to you in endeavouring to shew that the editors of the Medical Repository have confounded two diseases essentially different, may further strengthen this conclusion. To the examination and establishment of this point I now proceed.

PROPOSITION II.

That this fever and the yellow remittent are not “precisely the same disease.”

If the description given by Dr. Miller of the situation and police of New York be correct, it is matter of wonder and astonishment that it is at all habitable. Is this fancy, is it the exuberance of an imagination filled with images of septon in every pool of water, in every useful extension of solid surface? “Nothing can account,” says he, “for this local, stationary and *inexhaustible* poison, but the exhalations from the *masses of filth and pollution*, overspreading a large area of ground, forming *a vast hot-bed of putrefaction*, incessantly *teeming with miasmata*, and thereby in despite of currents of air, loading with

the seeds of disease every successive portion of atmosphere, that sweeps or stagnates over the pestilential surface." Edinburgh Med. Journal, No. 11. p. 284. Such hyperboles, such "shallow seeming of verbal phantoms," are unworthy the man who affects accurate observation, and acute discrimination. As well might he with the poet describe the approach to the infernal regions, and boldly declare at once that on the eastern shores of the Manhattan

Vestibulum ante ipsum, primisque in faucibus orci,
Luctus et ultrices posuere cubilia curæ :
Pallentesque habitant morbi, tristisque senectus,
Et metus et malesuada Fames, et turpis Egestas,
Terribiles visu formæ, &c.

Such exaggerations the venerable Mr. Pemberton very justly complains of, when in his letter to Dr. Wistar, he says, "I hope my recollection of facts and opinions may contribute to assist my endeavours to obviate the impressions which the speculation of some late writers on the subject may have occasioned; tending to degrade the salubrity of our climate, both at home, and

in foreign parts; the injurious effects of which are too obvious to enumerate." Add. Facts, &c. of the College of Physicians of Philadelphia, p. 4. How consistent is this account of Dr. Miller's with that of Dr. Charlton, president of the Medical Society of New York, whose testimony we must consider as fully as respectable as Dr. Miller's. In his letter to Dr. Hossack, 9th September, 1803, he informs that gentleman of the following particulars. "I have practised physic in this city since the year 1762. The fevers that have usually occurred in summer and autumn during that period, were intermittent, bilious remittent, and slow nervous; of late denominated typhus fevers. I never saw a case of yellow fever, in the course of my practice, before the year 1793." This gentleman declares that he always considered "the yellow fever" as a disease of foreign origin, and not indigenous, and that although in 1795, he signed the report of the committee of the medical society, which was in favour of the domestic origin of the fever then prevailing, yet he did so, contrary to

his own opinion, merely in his official capacity. The description Dr. Charlton gives of the disease, strengthens the propriety and justice of his opinion, for there cannot be a more exact picture of the malignant pestilential fever of Grenada. He concludes with this remarkable corroboration of his statement. "I never met a case of yellow fever in the country, but which could be clearly traced to have been derived by infection from the city." Ibid. p. 13, 15. A nearly similar statement is that of Dr. Samuel Bard, who began to practise medicine in New York, in 1766. He speaks of hospital malignant and jail fevers during the revolutionary war in New York, but never did he see a case of "yellow fever" till 1795, when the first case which occurred to him, that of Mr. Jenkins, he describes, and is precisely similar to the malignant pestilential fever of Grenada. "You will perceive," he concludes, "that I consider this fever as a distinct idiopathic disease, and not a variety or grade of any other." I likewise believe it to be a stranger and not a native of our country, and from the

best information I have been able to obtain; in every instance of accession imported from abroad." Ibid. p. 16, 19. Dr. D. Hosack communicates in the same collection, a very remarkable and important anecdote of Dr. Ledyard. "Dr. Ledyard, when he first entered upon the duties of the Health-office, as he himself informed me, went to Staten island, with the belief that the yellow fever was not an imported disease, but generated at home. A few weeks before his death he informed me that he had been compelled to change his belief, and that all his observations at the Health-office satisfied him that the yellow fever was exclusively derived from the West Indies." p. 32. On the other hand Dr. Miller asserts in his Report, "as the materials of putrefaction, and the degrees of heat, in a large city, greatly exceed what is found in the adjacent country; so the diseases arising under such circumstances must be proportionably more malignant. *The pestilential fevers of our city differ only in grade, from the bilious and remittent fevers of the country.*" But there is a remarkable circumstance which it

becomes Dr. Miller to explain, as related by Dr. R. Bayley, Med. Rep. V. i. p. 128, in his letter to the governor of New York, dated November 28, 1796. “ The docks spoken of in the south-eastern part of the city, *which were in so loathsome a state*, have been completed, and generally covered with a sufficient quantity of good earth, gravel or sand. The grounds have been drained, or where that was not practicable, the surface has been rendered uniform, with clean earth; the vacancies under the stores which were built on piles, have been filled up in a proper manner, and the wharfs have been kept free from rubbish and filth. In short, so much care and industry have been bestowed here to remove the nuisances that so generally abounded, that where a person was before almost suffocated with intolerable stench, one may now pass without experiencing the least offensive smell.” Here of course the fever of 1795, by Dr. Seaman’s account, originated; but in 1796, these nuisances being removed or obviated by the health-officer’s report, it became necessary to find out a new source of the fever which prevailed during the summer of that

year, and Dr. Bayley discovers it in a new dock erected on the south-west side of the city, where “there had been actually *two horses* buried in the rubbish, which had died in the spring.” This nuisance could not be removed, “because there was a contract between the proprietors of the ground and other individuals, for filling in that dock, with which *the magistrates did not think themselves authorized to interfere, unless it was declared to be a nuisance on oath, or by the presentment of a grand jury.*” How feeble their authority, or how “imaginary” the evil! In 1805 however, a fever appears, and as it must be endemic, Dr. Miller finds its source in the identical wharfs, slips and made ground of the south-east, which Dr. Bayley assured the governor had been rendered perfectly wholesome nine years before. These gentlemen must suppose their fellow citizens, and their readers in general to be incapable of discernment, thus to trifle with them. We see then plain unsophisticated facts, opposed to the most extravagant and violent amplifications of a few local circumstances which even an enthusiastic imagination is

puzzled to make out the sources of disease. We see a fair, honest line of distinction growing out of, and sanctioned by the experience of half a century, on the one hand; on the other, an incoherent hyperbolical jumble, which leaves us the dilemma of disbelieving respectable, and there can be no doubt, competent witnesses, or abiding by an absurdity in physics, that one cause has produced two effects totally opposite and essentially different from each other. A circumstance noticed by Dr. Charlton, and Dr. Ledyard, I can most decidedly attest to myself, viz. that though the city of New York was during the revolutionary war, more crowded and more filthy than ever it was at any other time, yet not a single instance of “yellow”* or malignant pestilential fever occurred, nor was it noted for any extraordinary sickness. But let us pursue the subject through another train of evidence.

* Let it be observed once for all, that wherever I make use of the word yellow as applicable to the malignant pestilential fever, I mark it as a quotation, in compliance with the general but very improper and incorrect adoption of the word by the public, by no means in conformity with the result of my own experience and observation.

In the case of imported infection at Newhaven, so amply and ingenuously related by the two Drs. Monson, the proofs were so strong that Mr. Webster, who has published Dr. Monson's narrative, cannot withhold his unequivocal assent, "it seems to be proved that in Newhaven the disease was introduced and propagated by infection." Collection, p. 233. This very Mr. Webster afterwards published in the Commercial Advertiser, a laboured refutation of every important circumstance, and attempted to produce a conclusion diametrically opposite, without the least apology for his change of opinion. Such is the insidious conduct of those, who being driven from the ground they vainly attempt to maintain, endeavour to regain it by subterfuge and crime. This is strong language, but it is fully justified by the proceedings of these men on the present occasion. The whole of the correspondence and statements are fully set forth by the College of Physicians of Philadelphia, and are very important documents in this controversy. They are distinguished by a very remarkable feature,---a palpable and egregious perjury is employed to in-

validate a fact supported by incontrovertible evidence. The college of physicians conclude the statement temperately and judiciously; "We are not disposed," say they, "to press this subject very hard, nor is it our intention to mark out any one as guilty of subornation in procuring the two unhappy men to forswear themselves; but we cannot in duty refrain from making a short appeal to our fellow-citizens on this statement of facts. What will they think of men, who to support a system pregnant with nothing but mischief, will go to such unheard of, such unsuspected, such criminal lengths. And will it not inspire a just and salutary caution, how they implicitly receive the interested evidence of captains of vessels which have introduced pestilence and death among us." Additional Facts, &c. by the College of Physicians of Philadelphia, p. 43, 71. Further comment is unnecessary. I shall only observe upon it, that this might be deemed sufficient to guard the inhabitants of the commercial towns of America against the unjustifiable and nefarious reports of supposed endemics,

for what sophistry can remove the impression made on our minds by the commission of "a crime so atrocious," says an amiable and christian moralist, "that one single perpetration makes a man infamous." This remark is applicable to the case of the *Zephyr* at New York, in 1795. This vessel sailed from a port of St. Domingo, which we are assured by unquestionable authority, was at the time the very abode of pestilence and death. There cannot be a doubt of the pestilential state of this vessel, and of her having been the true source of the pestilence which raged at New York in that year; she sailed from a port where the pestilence prevailed to such a degree as to make the inhabitants and garrison, drop, says Mr. Bryan Edwards, like the leaves in autumn. When she arrived at the wharf at the bottom of, or near Dover street, she was visited by the officer of health, who found on board of her three persons ill of fever, and the corpse of one who died that morning, the 20th July. On the 22nd, the officer of health was taken ill, and died on the 30th, with

evident symptoms of the malignant pestilential fever, called by the health committee a bilious fever of the malignant kind. On the opposite side of the same wharf, lay the William of Liverpool, till now a perfectly healthy ship, although at this wharf several weeks before ; the people of this and of the suspected vessel, called by Dr. Seaman, the Caroline, (although the Zephyr is evidently meant on comparing this gentleman's account with Dr. Smith's, but the mere name is of no importance) " were employed on this wharf at the same time, in unloading their cargoes." On 25th July, four persons of the William were taken ill with the symptoms of the fever of the officer of health, viz. " yellow skin, hemorrhagies, vomiting of black matter resembling coffee grounds, &c., and all died within seven days." The people of the Zephyr or Caroline, either remained on board or lodged at the adjoining public houses (similar no doubt to the grog-shops of Grenada.) On the 25th also, the owner of the ship Connecticut, lately arrived from England, and till now healthy, one of the

mates, the steward and two of the hands, were seized in the same way, and all, except the owner, died. This ship lay about two hundred feet from the Zephyr. The owner of the Connecticut was employed about the ship great part of the day, *but ate and slept in an airy part of the town. Persons were taken with the distemper, about this time, in other parts of the city, who had no connection with the brig Zephyr, nor with any other vessel.* “Fitch’s store stood on the next wharf to where the Caroline lay.” The clerk was taken ill on the 26th, “and died a few days after.” Dr. Smith says, “three persons who were in Mr. Fitch’s store, were taken sick and died.” Dr. Seaman, a strenuous but candid and reasonable advocate of the domestic origin of the fever of New York of 1795, says, “the circumstance of so many persons being taken on the same day, renders it highly probable that the contagion of this disease must have been introduced by the Caroline; since had it arisen from the pre-existing circumstances of the place alone, it is not likely that it would have

shewn itself in so many instances at the same time." These facts speak for themselves, they constitute a chain of evidence which the affidavit of Captain Comfort Bird, or the oath of any interested person, cannot break. Had the degree of weight of this affidavit been ascertained as in the case at Newhaven, it would, I doubt not, have been found, equally nugatory, perhaps equally criminal. Let these facts be compared with the maze of argument, the artful and elaborate attempt of Dr. E. H. Smith, to subvert them, or to paralyze their force. He shews himself, however, a good sophister; his arguments are ingenious, and "would pass in a school disputation, upon a thesis, but can have no influence upon a candid and rational mind." The *paralogy* is indeed, in the fervor of his enthusiasm, established by himself, for admitting the necessity of predisposition to the effectual action of morbid causes, which I presume no physician or philosopher will dispute, the following is decisive in favour of infection or contagion. "Infection may be brought into any place (and therefore

into this city) from abroad ; *under certain circumstances of the place*, where it is introduced, it becomes very active and destructive ; but when *these circumstances* do not exist, however the person immediately affected, if it be introduced by a sick person, may suffer, it is harmless, so far as the general health of that place is concerned." What more is required for the admission of pestilence. It was under " these certain circumstances" the filth and nastiness of the inhabitants, and probable irregularities of the garrison, the infection brought into Cadiz by the crew of an American ship, (London Chronicle, September 23, 25, 1800) or into Gibraltar by the fugitive Spaniard, Sancho, became active and destructive. See Webster's Collection, 55, 115, Med. Rep. V. i. p. 326, 329. &c. The uncertainty and wavering state of Dr. Seaman's opinions on this most interesting point, is manifested in several passages of the paper I have here quoted from. He begins with invective and sarcastic reflections on a gentleman who has highly benefited his country, by being among the first

to *penetrate* into the true causes of the pestilential evils they have laboured under, and to point out the means of preventing them, and this it would appear merely because he holds, on good grounds, a different opinion, p. 315; he begins to lose sight of the purpose he began with, in p. 325, where he is in doubt what the cause has been, septon or hydrogene or other peculiar principle of putrid effluvia, “*or whether, merely like a smoking hot combustible, it burns only after having received a spark from elsewhere.*” In 326, he makes an awkward concession to the gentleman he so bitterly inveighed against, and adds “however from subsequent enquiry” (the acknowledgement is candid and does him honour) “and more minute information” (did he too confine his enquiries merely to the *superficial scum* of newspaper observations?) “in regard to some circumstances, *not then so generally known*” (so Dr. Currie’s *learning* has surpassed his!) “it now appears to me probable that a foreign fomites might at first have excited our pestilential vapours into the action that spread such devastation in the most afflicted

part of our city." He is further confirmed in p. 327, and his conviction seems to be complete in 329. "I am rather inclined to believe, that generally in our city, it has been set in action by an assisting cause from abroad." The curious circumstance mentioned p. 330, should not be overlooked, and it is to be hoped Dr. Seaman is now a firm convert to the cause of truth and humanity. If the mud-machine or mud-turtle as it seems to have been emphatically named, produced no "yellow fever" by its "pestiferous purgations" on the "filthy" slips, in 1792 and 1793, p. 330, how came it afterwards to have this baneful effect? p. 317. Such agitation, such facility to the evolution of the "peculiar principle," septon or hydrogen of the putred effluvia of these pestiferous slips, one would imagine would kill at a stroke, would be, without a figure, ἡ θανασίμη πλῆγῃ, and yet we are told when this agitation was suspended in 1794, when it was afterwards entirely relinquished in 1795, and 1805, the pestilence raged with almost unbounded violence. The great champion of the

endemic party, Dr. E. H. Smith, is inconsistent with himself; compare the extract made above from Webster's Collection, p. 104, with the declaration so often repeated in the same paper; "No direct, no clear evidence ever has been or can be produced in favour of the opinion, that the fever was imported." p. 94, 95. These facts may be sufficient to establish the general position, that in most of the ravages of the "yellow fever," so called, in the maritime towns of the United States, imported infection may be considered as the cause, and not the local circumstances of these towns, which are not peculiar to *them*, but found existing in all sea-ports of all countries and climates, never however, as the causes of destructive, contagious, pestilential fevers. In those instances, which have occurred during the same periods, of the appearance of epidemics in inland and remote situations, I believe the rational and unprejudiced enquirer, will find that in general at least, the cause has been local, and readily discovered in the exhalations from swamps, of which in warm climates, the yellow re-

mittent fever, and in all climates, intermittent fevers and their modifications, are the product. That this, in these situations, has been invariably the case, we do not find. This is remarkably exemplified by Dr. James Stratton in a letter to Dr. S. P. Griffiths, dated Swedesburgh, December 31, 1805. “In the year 1797, I saw seven cases, all these were inhabitants of this neighbourhood, five of them had been to Philadelphia; of these four died: *two cases of the disease being communicated*, one died with all the distinguishing symptoms which accompany the most unfavourable cases, the other recovered.” The fomites of infection seem to have been a quantity of oakum brought from Philadelphia by a third person, who also died. The most extensive diffusion took place in 1798, when forty-two cases occurred to Dr. Stratton; of these nineteen died, twenty-seven had retired with the disease from Philadelphia or Wilmington, and fifteen received the infection by communicating with them. One person, a young lady, fled from Wilmington to the opposite shore of Jersey, was attacked about a week after her flight, communicated the disease to her

uncle, her nurse, and a young man who visited her. Two of the four died. Dr. Stratton concludes from these and other instances which he details, “ that the yellow fever has never appeared in this part of the country, except in those seasons, and when it had previously existed in Philadelphia; that there have been many instances of its being brought from Philadelphia to Jersey, and of its being communicated from the persons thus infected to others.” If the disease may be brought from Philadelphia into New Jersey, he concludes, “ I see no difficulty in supposing it may be brought from some other place to Philadelphia.” Dr. Eneas Munson, in a letter to Dr. William Shippen, dated Newhaven, May 3d, 1805, mentions several instances of imported infection. “ I could mention a number of solitary cases of its introduction since, and those decided and clearly marked; *but what shall I say more? Through all my pilgrimage in life, which is upwards of seventy years, I have never met with any of the human race more blind than those who were determined they would not see.*” These are positive circumstantial facts

opposed to the assertion of Dr. Miller. "In no instance did these victims of the epidemic communicate the contagion." But I shall notice this discordancy hereafter. See Add. Facts, &c. p. 42 and 72.

The confounding these opposite causes of fever, the exhalations from marshes, and contagion, has unhappily given rise to the diversity of opinion, and consequent rancorous animosities which have prevailed in North America; and in relation to myself, to the wanton obloquy of the late Dr. E. H. Smith: it has induced Dr. Rush to declare that "Dr. Chisholm stands alone among modern physicians, in maintaining a contrary opinion," and to attempt, by fallacious ratiocination, to explain away facts which he otherwise could not dispose of. Med. Rep. v. 6. p. 156. It has excited the ingenious research of Dr. Mitchell into "the septon of the perspirable fluid adhering to the shirt and other raiment of sailors in the West Indies," and of dirty bills of paper money, for the cause of "yellow fever." Med. Repository, vol. 3. p. 167, 168. Heaven and

earth have been ransacked in the laborious pursuit; the phenomena of nature have had their share of aggression assigned to them; and finally, it has drawn from his nervous but insidious pen, the criticism of Dr. Miller.

Hinc mihi prima labes : hinc semper Ulysses

Criminibus terrere novis : hinc spargere voces

In vulgum ambiguas, et quærere conscius arma.

Virg. *Æn.* 2. 97.

Before I close this part of my argument, I shall request your attention to the conclusion of the College of Physicians of Philadelphia in 1798, as established by ample observation in 1805.

The College of Physicians of Philadelphia, acknowledged by all parties to be a respectable and enlightened body of men, declare solemnly their conviction, 1st. that the “yellow fever” (so called) “differs essentially from any other disease which is common to North America, and agrees in its most essential symptoms, with what is called the yellow fever in the West Indies. 2d. That it has been regularly traced to the vicinity

of some vessel or vessels from the West Indies; or to persons or clothing connected with them. 3d. That the principal peculiarities of this fever are its contagious nature, the progress of the symptoms, and the mortality consequent on it. 4th. That to prove the contagious nature of this disease, would be equally useless as to prove the contagion of the plague. 5th. That in all their observation and practice, they know of no case where the autumnal bilious remittents of their country have proved contagious. 6th. That although these are sometimes attended with violent and dangerous symptoms, this striking characteristic of contagion being always absent, they never become an object of public dread or concern." These corollaries are preceded by these pointed questions: Where do we see the first appearance of our pestilential fever? Is it amongst the marshes to the southwest of our city, or in the neighbourhood of our wharfs? Is it in the confined alleys, or on the salubrious banks of the Delaware at Kensington? Is it not always near those places where vessels from foreign countries are found? Do the fevers

common to the country, steal on insensibly, infecting one person after another in a family and in a neighbourhood? Are they equally severe in seasons so opposite as 1797 and 1798? They afterwards remark the very proposition I am contending for, viz. “that very erroneous opinions on this subject have arisen, from confounding this pestilential fever with the malignant remittents of the West Indies and America;” and subjoin some remarkable proofs immediately applicable to the importation of infection in 1798, among which is the case of the ship *Deborah*, the prevarication if not perjury of whose captain is proved by the return of sick by the steward, and by the information of the mate, and of two of the citizens of Philadelphia, and more especially by the letter of Dr. Bonneville to Dr. Griffiths. These were the sentiments and statements of the College in 1799. See *Facts and Observ.* p. 13—40. A lapse of seven years has served only the more firmly to establish the validity of them, by furnishing additional and most important facts and observations. The fatal epidemic of the year 1805 in Philadelphia and New York, has

presented abundant evidence of the truth and stability of these conclusions. What is opposed to the strong and stubborn facts published in the New York public prints of that year, supported by the authority and experience of the College of Physicians of Philadelphia, but the wire-drawn arguments, the subtle tissue of sophistry, exhibited by Dr. Miller in his Report to Governor Lewis, the awkward attempt at concealment of the health officer, and the palpable dissimulation and empty profession of the president of the Board of Health? Have any of these attempted to marshal facts against facts? On the contrary, has not Dr. Miller, imitating the example of his former colleague, Dr. Smith, in 1796, glossed over facts which did not admit of explanation favorable to his own most unfortunate, most mischievous bias of sentiment? When the order of the mayor of New York rendered refusal of admission into the Marine Hospital on Staten Island to Drs. Hosack, Williamson, and Stringham, impossible, was not the most singular and unexpected attempt made to exact from these gentlemen a preliminary stipulation of con-

cealment? What purpose could the adoption of so extraordinary a measure, at the commencement of the epidemic, be expected to fulfil but concealment of the truth? And why have recourse to arts so unbecoming, were there not sinister objects to be obtained*? The truth is, to use the strong

* New York Evening Post, December 24th, 1805, Dr. Williamson to Dr. Hossack thus states this remarkable fact: "When Dr. Stringham and yourself were at the breakfast table with Dr. Rodgers, one of you expressed a wish to see the hospital, Dr. Rodgers observed, that he had been ill-used by some person (I think he was not a physician) who had been suffered to visit the sick. He complained, if I mistake not, of misrepresentations by that gentleman. He went on to express his doubts of the propriety of suffering us to visit the sick. In a little time he was more explicit, and said we could not see them, unless we claimed admission by authority. I was greatly disappointed by that declaration; for you may recollect, that when we landed from the boat, you offered me the letter from the mayor to be delivered to Dr. Rodgers to secure an admission to the hospital; that I told you I would not affront Dr. Rodgers by handing him a pass, for I never had seen nor heard of a case in which a physician refused to let a medical gentleman visit any hospital under his care. You thought differently on the subject; and I took the letter in confidence of not having use for it. While we were on the road to the hospital, Dr. Rodgers once and again repeated his expectation, that

language dictated by a mind conscious of the rectitude of the cause it goes to support, “ were this, and certain facts connected with it, made known, I venture to predict that that public indignation which has been so artfully, unjustly, and wickedly excited against this gentleman, Dr. Hosack, would be transferred from him to the heads of those on whom alone it ought to fall, those who did conceal cases of malignant fever for two, three, four, and five days in the very heart of the city, regardless of the consequences upon the health and lives of the rest of the community, and in defiance of the public statute.” And “ to talk of quarantine laws

nothing he might say should be repeated or published. You appeared to be offended by the request, but you did not, that I could observe, make any reply to his injunctions; and it was clear to me, from your silence, that you would hold yourself under no restraint.” The editor of the paper introduces these letters, by observing, “ that though the dispute is personally between Dr. Rodgers and Dr. Hosack, yet as it relates to the conduct of the public health officer of New York, at a crisis when the utmost alarm prevailed through the town, it must excite the attention and awaken the sensibility of all classes of readers. As to the judgment to be formed, every one is left to frame his own, unbiassed by me.”

which retain the vessel at a distance, and permit the diseased hands and the infected clothes and bedding to be introduced into the city, is mocking and nonsense." New York Evening Post, November 14, 1805. Does any thing contained in Dr. Miller's Report invalidate the correctness and cogency of the following statement? "One great error (of the quarantine laws of the state of New York) is leaving so much to the discretion of the health officer, without knowing what his theories or his notions may be, another is leaving so much open to construction by the captains of vessels. As to the first it is entirely submitted to the health officer what and how much of the wearing apparel, bedding, and every thing on board, is *likely* to communicate infection, in order to its being landed and cleansed. But how much would such a provision be worth, if a man should happen to be appointed health officer, who in fact disbelieves altogether in the communication of disease by clothing or bedding, &c. Where is our security then, where our commercial existence then? As to the other objection, I shall explain myself

in a word. The present statute only quarantines vessels where a "malignant or pestilential fever *prevailed* at her departure." Now what is the meaning of this? What is a malignant or pestilential fever? A captain will swear that such disease did not prevail, and save himself by saying he does not mean the yellow fever, which he does not consider as such; and in this he may find support too from some of our own medical writers. Again, what is the meaning of a fever's prevailing? How many cases must happen before it can be said to prevail? In a matter of this vast importance, so much latitude for construction should not be left open."* If the health officer enjoys a revenue of from 15 to 20,000 dollars a year from visitation fees, whether it is ascertained that the pestilence is fairly introduced into the city of New York or not, where is the stimulus to keep out the disease? The health officer is thus made a destructive exemplification of the miser's soliloquy.

——Populus me sibilat; at mihi plaudo

Ipse domi, simul ac nummos contemplor in arcâ.

Hor. Sat. 1. 1. 66.

* See New York Evening Post, Nov. 26, 1805.

If we extend our investigation to the cause of the epidemic at Philadelphia of the same year, we shall see similarly unyielding and important facts opposed by the same delusive ratiocination. We shall see the Report of the Philadelphia Board of Health addressed to Governor M'Kean, manifesting the introduction of Infection through the clandestine visit of two persons to the Lazaretto, where at that time (July 1805) several vessels from different ports of the West Indies were performing quarantine, on board of which persons had been sick, and had died of the yellow fever, and particularly the schooner Nancy from the city of St. Domingo with a number of wounded French soldiers who had been received on board from a military hospital, soon after which the whole crew, excepting two, became ill of the yellow fever; one died at sea after three days' illness, and the captain and one seaman were landed dangerously ill at the Lazaretto, according to an extract from the log-book of the vessel transmitted to the health office. We see this board tracing the epidemic in the clearest manner from this source from which it propagated itself "in the

same manner as diseases universally acknowledged to be contagious, are usually propagated, and not like a disease derived from noxious effluvia diffused in the atmosphere to numbers in different directions at the same time, nor like a disease occasioned by foul air proceeding from a particular source, which necessarily ceases to spread, or even to exist, soon after the original source from which the foul air proceeded, has been removed or corrected." How are these disturbers and destroyers of society, the pertinacious theorists of New York and Philadelphia, to be execrated by their fellow citizens, when they perceive that a mortality of 300 out of little more than 800, is ascribed by the Board of Health to the prevalence of these "wild and mistaken notions as to the origin and nature of the disease;" when they hear the same board lamenting that their "measures so well calculated to prevent the disease from spreading could not have failed of success, as was proved by the proceedings in Providence Rhode Island, *if the people had not unfortunately been impressed with an opinion that the disease was not contagious, and of*

course that no precautions were necessary. Misled by this opinion they disregarded the regulations and admonitions of the board, in consequence of which the disease was gradually conveyed into other families, and into streets more remote, and though the increasing mortality soon convinced the people that the disease was of a different and more dangerous nature than they had been taught to believe, and occasioned the precipitate flight of multitudes, it had become too widely extended before this conviction took place, for the subsequent regulations and exertions of the board to eradicate it or entirely suspend its progress, &c." See New York Evening Post, December 27, 1805, and Additional Facts and Observ. by the College of Physicians of Philadelphia, 1806. The foregoing observations receive confirmation from the highly important communication of a well known physician formerly of Bath, and no doubt of your acquaintance, Dr. Anthony Fothergill. His correspondent, the worthy and respectable American consul, resident at Bristol, Mr. Elias Vanderhorst, has obligingly enabled me to avail myself of the

observations of Dr. Fothergill, contained in a letter of this gentleman's to the consul, dated Philadelphia, December 20, 1805, and also to state the authority, which must be considered as indisputable. "You have heard, no doubt, that in the beginning of the last Autumn this city and New York were visited by that terrible malady the yellow fever, which prevailed through September, October, and part of November, till it received a check apparently from a cooler season setting in. It remained for some weeks a local disease in the southern suburbs, but at length was communicated to several of the principal streets in the city as far as Eighth Street westward, but chiefly infested Water Street, Front street and the margin of the Delaware, but providentially the mortality was not near so great as in the former epidemics. Many of the professors and medical practitioners here, deny that the disease is contagious, and in this notion the *body of merchants* bear them out; hence the quarantine laws are but very superficially executed, and it is reported, they will next year, be either curtailed or wholly abolished. But nothing has

been yet advanced among the numerous publications on the subject, that seem sufficient to convince any impartial or unprejudiced reader of the rectitude of their opinion; nor will they be able to persuade the ablest physicians of Europe that the disease is free from contagion, or that quarantines are superfluous. Quarantines, it is true, fail of preventing its entrance into cities in America, or of cutting off the means of communication. Why? because the laws of quarantine are continually eluded, and commercial interest preponderates over the public health. The quarantine laws therefore, instead of being further relaxed, ought to be more strenuously enforced. This I am persuaded would redound to the health of the maritime cities and also to the true interest of the mercantile world at large." In another part of this letter it is remarked "England has hitherto escaped this dire contagion, in a surprizing manner, but how long it will do so after *it* has been communicated to Cadiz, Malaga, Andalusia, Leghorn, Gibraltar, &c. &c. is much to be doubted, especially if the quarantine laws in this country become a

dead letter." Dr. Fothergill also believes with me, that the yellow remittent and "yellow fever" (malignant pestilential) are often confounded. "A bilious remitting endemic fever at Savannah, and in some of the West Indies, though often fatal, is not considered as contagious; and *though often mistaken for the malignant yellow fever of America*, appears to be of a different nature." This eminent physician (F. R. S. and member of the London College of Physicians, you know) lamenting the calamity under which the inhabitants of the American cities have been so cruelly oppressed, and wishing to alleviate where he could not prevent, drew up some very judicious regulations, which he entitled "Friendly Cautions on the Prevention of Pestilential Contagion, &c. written during the ravages occasioned by the yellow fever of 1805 at Philadelphia, &c. and submitted to the serious consideration of the inhabitants." These were published in Poulson's American Advertiser. Whether these judicious observations have induced the gentlemen of New York to number Dr. Fothergill among those European physicians so honorably distin-

guished by the epithets “rigid and intolerant,” I know not. Whether this may be or not, the letter I have been permitted to quote from must be considered as particularly meriting the confidence and belief of the public, from the circumstances under which it was written, merely for the information of his friend, Mr. Vanderhorst, and under no influence whatever but truth and conviction.

It is astonishing how decided and authoritative those often are who have formed their opinions of a disease before they have seen it. This is deeply to be lamented, because it is productive of infinite mischief. I may here also observe a circumstance remarkable in itself, and of some importance in the present enquiry, viz. that the principal officers, with few exceptions, of the Hospital Staff, which left England with Sir R. Abercrombie’s army for the Leeward Islands, and Ireland with General Whyte’s for St. Domingo, in 1796, believed in the endemic origin of the disease generally known by, but improperly called, “yellow fever,” without having at any one time had an opportunity

of seeing the malignant pestilential, or yellow remittent fever: whilst all the officers of the old staff, who had been in the West Indies for periods of longer or shorter duration, in almost no instance less than a year, and actively employed in the treatment of these fevers, held an entirely opposite opinion. It is further deserving of particular notice, that among the few exceptions I have mentioned, those were most conspicuous, whose residence in the West Indies had been the longest. These remarks have no doubt, in some extent, occurred to yourself; and I am confident you have especially noted the venerable and esteemable Dr. Wright's opinion, which goes to the establishment of the foreign origin of the pestilence, and of its contagious nature. The solicitude of the endemists is very singular, as well as the wonderful lengths they have gone in sophistry and speculative argument. What conclusion is to be drawn from this? I leave you and the public to judge. In some instances the prepossession I have mentioned, being given to the public in a very imposing and specious garb, has no doubt done infinite mischief. Of these may

he mentioned Dr. Pinckard's book, denominated "Notes on the West Indies." This gentleman was ordered to accompany the detachment of troops destined to seize and occupy the Dutch colonies of Demerary and Berbice, soon after his arrival at Barbadoes from England. There he met with the yellow remittent fever, the endemic of the climate, during the more unhealthy season of the year; and this he has considered the "yellow fever," so called of Grenada, and of all the West India islands and of North America, of 1793 and the subsequent years. It is not my purpose to censure any hasty opinions which may have been formed by this ingenious gentleman; but I cannot pass over his own acknowledgment, that "after a residence of so many weeks in the West Indies, he had seen only a solitary example of that disease; for amidst all our sickness," he continues, "and crowded hospitals, (at Barbadoes) only a single instance of "yellow fever" has hitherto occurred." V. ii. p. 143. He has devoted a whole letter of his amusing and excursive "Notes" to this subject. You will please to recollect, that in the account

I have given of these colonies, Essay, V. ii, p. 201. I have taken notice of the only instances ever known of the introduction of pestilence into the colony of Demerary prior to the year 1800, and have particularly stated the stations more destructively affected by the endemic yellow remittent fever, and have even noted the mortality which this was the cause of in the garrison, during the year Dr. Pinckard's Notes are said to have been made. In the instance of sickness at Mahaïeka, at all other times a very healthy situation, there can be little doubt, from a variety of considerations, that the infection of the fever of the St. Domingo army at the Cove of Cork was the cause: and indeed the remarks of Dr. Pinckard relative to the nature of this sickness, seem to give great force to the information I received in that colony, "Notwithstanding," says he, "our strict observance of cleanliness, and all our care to prevent disease, I am sorry to remark, that numbers of the soldiers fell sick on the passage (to Demerary): but in mentioning this, I ought to observe, that the 39th and 99th regiments were mostly composed of draughts

from various other young regiments, consisting principally of old men and unseasoned boys, who had been collected together, not according to the best regulations of the recruiting service: and also, *that multitudes of them had been sick on the passage from Cork to Barbadoes, consequently many (of those now ill) are only suffering from a relapse of former disease.*" V. ii. p. 168. In a preceding part of this volume, "the malady which now exists," he says, "has been brought with the troops from the Cove of Cork." It is the common hospital or ship fever, &c. p. 7. There are circumstances too, noticed by the Doctor, and which occurred at Mahaïeka, which, however lightly he may treat them, may certainly, without the implied imputation of credulity, be received as explanatory and even illustrative of this. In V. iii. p. 79—81, he mentions the prejudices of the inhabitants having led them to imagine, that if the sick at Mahaïeka were treated by *the colonial doctors*, the disease might be easily subdued; and that, in consequence of this, he requested the aid of the medical men of the colony. Several of these

gentlemen gave their medical attendance; and one of them in particular was greatly disappointed and chagrined at the unhappy result of four cases which he treated in consultation with the author, “ declaring that there was *a something different* in the fever which prevailed among the troops, from that which usually attacked the colonists.” However incompetent the practice of the *colonial doctors* may have been in the yellow remittent fever, and that it was so, as formerly conducted, I do not hesitate to aver, yet in the cases of fever at Mahaïeka, neither the practice of Dr. Pinckard, nor their practice, was of any avail, as appears by his own very candid and fair representation. That the fever was different, or that there was *a something different*, in the emphatic expression of the *colonial doctor*, there is not a doubt, for it is confessed on all hands; and being so, the inefficacy of the colonial, as well as the military hospital practice of Dr. Pinckard, cannot merit any imputation of ignorance or neglect on either part; for in truth, neither had seen the disease; and Dr. Pinckard had only seen one solitary case of yellow remittent

fever in his life before his arrival at Demerary. What Dr. Pinckard says respecting the origin of the malignant pestilential fever at the Cove of Cork, cannot be considered as relevant; indeed no importance can be attached to it, for it is merely hearsay. The facts I have stated, Essay, V. i. p. 203—7, go to the proof of its origin being derived from the West Indies; and these, in conjunction with the observations I had myself an opportunity of making in the hospitals on Spike Island, have removed all doubt from my mind on the subject. All Dr. Pinckard's arguments must therefore be considered as conclusions without established premises; and of course what I have generally said in the preceding sheets, and in my Essay, may be received as their confutation. There is one argument, however, which I shall take notice of, as it seems more plausible than the others, and will therefore be more apt to mislead. V. iii. p. 416, he says, "When Europeans first take up their residence in the West Indies, it is usual for them, sooner or later after their arrival, to undergo an attack

of fever, which in times of peace and tranquillity, when, as they are called, the newcomers are but few, is termed a seasoning fever; but in times of war, when, from great multitudes arriving at the same period, its destructive effects are more striking, is baptized with the terrific name of “yellow fever:” but whether denominated seasoning, yellow, Bulam, or Siam, or marked by any other appellation, it is only the common bilious fever of hot climates: and it appears under an intermittent, a remittent, or a continued form, according to the soil and situation of the place; or the habit of body, and other circumstances of the person attacked.” This is precisely and truly applicable to the yellow remittent fever: but in relation to the malignant pestilential fever of 1793, 4, 5, and 6, it is absolutely irrelevant and untrue. Was the fever epidemic at Philadelphia, New York, Newhaven, and the other cities and seaports of North America, during war in that country? Was the town of St. George’s, Grenada, and the other towns of the West India islands, particularly crowded when this pestilence

appeared first in them? Except Barbadoes and Martinico, none of the islands was marked "by great multitudes arriving at the same period;" and until 1794, nothing of this kind occurred at these, and yet the malignant pestilence was prevalent at both in 1793. It is most particularly desirable that ingenious but speculative medical men would well consider the subject they undertake to write on, before they commit their crude and undigested thoughts to the public. If their object is amusement, these speculations may be innocent; if instruction, they will be productive of mischief in direct proportion to the importance of the subject. Hence, when a physician, filled with vain notions of self-importance, and superior attainment, descants on the cause, symptoms, and treatment of a disease he knows nothing about, he may give a bias to the mind of his reader fatal to him, by its tendency to remove from his mind apprehension of danger, when danger can be avoided, only by the adoption of useful and effectual means of prevention. You will readily per-

ceive it is for this reason I have more enlarged on the speculative opinion of this ingenious medical gentleman, than it might otherwise merit; for my way of thinking in these matters is, “*Non mihi sapit qui sermone, sed qui factis sapit.*”

PROPOSITION III.

That my deduction of the disease from the pestilential state of the ship Hankey is just, correct, and supported by evidence corroborative of that which I received from Mr. Paiba.

THE result of a very attentive consideration of the circumstances of the ship Hankey, as they were represented to me, on the arrival of that ship at Grenada, I have already stated to be, “*that a fever, proceeding originally, perhaps, from the inclemency of the season, and the circumstances of the situation of the adventurers, had become by confinement, filth, consequent impurity of air, and depression of spirits, a true jail fever, or a fever of infection heightened to an almost pestilential violence.*” Making this then my “*orationis argumentum,*” I shall proceed to the establishment of my

third proposition. In the course of my investigation for this purpose, we shall find that all difference between my statement of the situation of the Hankey, and that described by the more intelligent colonists of Bulama, exists in the expression of, not in the facts themselves.

Non ita sunt dissimili argumento, sed tamen
Dissimili oratione sunt factæ ac stylo.

Ter. Andr. Prol.

Captain Philip Beaver, of his Majesty's navy, who had the charge of the Hankey from England to Bulama, and afterwards had the principal share in the direction and management of the colonists at Bulama, particularly after the departure of the *Calypso*, has published a most complete and circumstantial narrative of the whole of their proceedings, and this he has done with a candour, a naïveté, which stamps it with the seal of authority and truth. Capt. Beaver's "African Memoranda" I have consulted, and the attentive perusal has impressed my mind, if possible, with a more full conviction of the deplorable and pestilential state of the Hankey,

than I possessed before. I shall endeavour to convey an adequate impression of this to your mind; and as I imagine the only doubtful part of the melancholy history of this ship, is that which preceded her arrival at Grenada, to that I shall chiefly confine myself. In this are to be found the remote causes of the dreadful fever which has devastated the West India islands, the British army and navy, the principal cities and towns of North America, and some of the more populous sea-ports and towns of Spain in Europe. These again are discovered in the events which led to the fatal termination of the Bulama expedition; and they are stated in a very clear and manly manner by Capt. Beaver, the whole of whose conduct reflects the highest honour on him. He committed only one fault, and that was in engaging in a scheme founded in error, and executed without judgment. These causes, he says, "are discernible either in the errors committed in Europe, or in those committed by the people on board the Calypso, which cannot be exceeded." p. 314. Nay, such was the infatuation universally prevalent

among the colonists, that the actions of almost every individual were so ill advised, as to do no good, and so ill timed as to do the most possible harm. p. 112. These errors appear to have been, the advanced period of the season when the proposals of the Bulama Association were first published, for when the ships arrived at Bulama the rains had actually set in:—increasing the number of the council, without adding any thing to their ability; thereby augmenting the means, and the probability of weak measures, and disunited counsels:—their conduct had been so thoughtless and ill-judged, that whatever influence they might have had at first, was before the arrival of the ships entirely lost, and the inevitable consequence resulted, universal anarchy and confusion, p. 47.:—the sailing without a charter, and consequently undertaking the enterprize without having any legal authority “over a class of men who certainly required legal restraint;”—a class of men characterised by the epithets, restless, turbulent, indolent, dissatisfied, “a licentious rabble,” “a motley assemblage of unthinking mortals,” “dissipated vicious

characters, and some most infamous," p. 22, 23, 152, &c. the neglecting to carry out the frames of buildings to shelter the colonists during the rains, one of the most fatal errors—the number of women and children who occupied half the space allotted for the colonists on board the Calypso and Hankey, the immediate effect of which was a want of room and free circulation of air. These are stated (p. 293 and 308—315) as the principal causes of the evils which took place, and perfectly sufficient they surely were. The immediate evils, were “the weakness unexampled, and absurdity unparalleled, which directed the proceedings of that part of the community which embarked on board the Calypso, which led to the adoption of the most fatal measures;” to which were joined, “*the fever which already had made some havock on board that ship, and the rainy season.*” After the departure of the Calypso, “it will be sufficient,” says he, “to mention *that dreadful, that melancholy mortality, which so soon swept away our numbers,* and rendered them incapable of doing much more than merely keeping pos-

session of the island." These causes, and their consequent direful effects Captain Beaver most amply and ingenuously exemplifies throughout his Journal.

That a body of people thus constituted should suffer by every species of calamity is not surprising, that infection should be generated and diffused among them is a natural and an expected result. Accordingly we are informed that a fever had broke out among the singularly turbulent people of the Calypso soon after the attack of the Cannabacs on 3rd June, 1792, and shameful retreat from Bulama, but before the junction of the Hankey. The origin of this fever on board the Hankey is thus clearly stated: (p. 54) "when I quitted the Hankey on the morning of the 5th, (June, 1792) I had left a quiet, clean, healthy and orderly ship, the Colonists contented and in good spirits; but when I returned on the 7th, I found a noisy, dirty, disorderly ship, the Colonists dissatisfied and dispirited. That such a change could have been operated in so short a time was scarcely credible, but such was the effect

of the *Calypso's* rejunction. The fever, from which the *Hankey* was still free, had already made its appearance in the former ship; and instead of separating the infected from the well, and taking any steps to prevent the spreading of that dangerous disease, by prohibiting any unnecessary intercourse between the two ships, the whole time, since the arrival of the *Calypso*, had been taken up in the constant interchange of visits: nay, the affected themselves, the very persons who had the fever on them at the time, had been actually on board the *Hankey*; and the consequence was that many days did not elapse before the fever made its appearance in that ship also. Nothing was heard but mutual reproaches from the people of the *Calypso*. The Colonists accused the members of the council in that ship, of a want of attention to their comfort and accommodation; they were tired with the length of the voyage, irritated with sickness, the loss of their associates, and the disappointment of their hopes; and became extremely dissatisfied with their situation; these general and reciprocal complaints, produced in the minds of a few, con-

tempt; but in the majority of the Colonists, despondency." (p. 82.) On the rejunction of the ships "their hopes, hitherto as sanguine as when they left England, thus suddenly checked, produced nearly as much discontent in the Hankey as in the Calypso. There was a settled gloom on the countenances of all, which it is difficult to convey an idea of, and which it was melancholy to behold. In this situation, to rouse them to action and energy was impossible." The infectious nature of this fever is particularly stated by Captain Beaver in several parts of his Journal and narrative (p. 54, 82, &c.) On the 19th July, the Calypso sailed for Sierra Leone and England, leaving the Hankey at Bulama with forty-eight men, thirteen women, and twenty-five children, Colonists, four seamen belonging to the cutter, and the crew of the ship, the number of which is not stated; for the government of the former, Captain Beaver drew up an excellent body of regulations, and for their protection from the rains, a house or covering was built over the ship. (p. 91, 109.) The sick list soon increased, for on 16th August, it amounted to twenty-

one. More than half the number of Colonists were sick on 30th September, and on 8th October, there were forty sick out of sixty-two, on which occasion Captain Beaver remarks “every body much depressed, not a soul capable of exertion.” Three days after he again remarks, “I have long since seen that not only from sickness, but *from a kind of stupor*, general depression of spirits, and total unconcern even for their own safety, has *in a most remarkable manner* seized every person in the colony.” (p. 143.) On 13th October, he says “every Colonist seems deprived of his faculties.” (147.) Such was their reduced state that on the 23rd October, only six hands could be mustered to work, and of these only two colonists, two sailors of the cutter, and Beaver himself. On the 23rd of November, the Hankey sailed from Bulama; on which day there were thirty-five sick out of fifty-three, and of twenty-eight Colonists, who remained with Captain Beaver on the island, twenty-three were sick. Fourteen Colonists embarked in the Hankey, viz. six men, four women, and four children. Such was the dreadful mortality attendant on this

fever, that out of seventy-five Colonists, thirty-five died from the 1st August to the 23rd November. Its nature must have been most terrible, for Captain Beaver mentions many who died of a few hours' illness. It also appears to have been direfully contagious, for, in one instance particularly, three persons who were near his bed during his own illness, from which he almost miraculously recovered, received the infection from his person, and died soon after. (171, 2.)

It is proper to observe here, that much of the inference made from Mr. Paiba's narrative by Dr. E. H. Smith, (Med. Rep. V. i. p. 477.) viz. that it was highly improbable that infection could be attached to or remain in the Hankey, arose from Paiba's stating that fever was not the only disease, and that under that title were included every form of fever, &c. among these ill-fated people. That this is an inference from premises which did not exist, appears from the lists published by Captain Beaver, in the appendix to his "Memoranda." (p. 433, 445.) By these lists it is evident; 1st. that of one hundred

and fifty-three embarked at Gravesend on board the *Calypso*, fifty-five died, or 1 in 2.43. 2nd, that of one hundred and thirty-two embarked at Gravesend, on board the *Hankey*, sixty-four died. 3rd. that of six in the cutter *Beggar's Bennison*, four died. 4th, that of three children born, three died. 5th, that consequently of two hundred and ninety-four, one hundred and twenty-six died, or nearly one half. 6th, that of "fever" so expressly said, forty-eight died, of which number more than thirty died on board the *Hankey* at Bulama; and 7th, that of the same disease with almost certainty from the circumstances stated, although not so expressed, nineteen died, in all sixty-seven of fever. Now if it is asked what were the causes of this fever, and mortality? All the causes which generate infection and infectious or pestilential fever, is the obvious answer, I apprehend, after an attentive consideration of the facts stated. In every instance of great mortality on the Coast of Guinea, and in many other countries of the old and new continents, within the tropics, and there are many dreadful ones given by

Dr. Lind and others, the cause has evidently been Marsh Miasmata, or an “inland impure atmosphere loaded with stinking sulphureous mists.” Nothing however of this kind is mentioned as having existed at Bulama, which is every where surrounded by sea, is no where marshy, gradually rises to a moderate elevation immediately from its shores, is blessed with abundance of running water, and with “a soil rich and prolific” affording ample pasturage to innumerable wild animals. In a word, to use Beaver’s emphatic expression, “here reigns abundance of every thing requisite to the comforts of savage life, and it seems to have been produced in one of nature’s happiest moods.” p. 341, 372. The sensible remarks on this “rather delicate” subject made by Captain Beaver himself, throw abundant light on it. “It may be asked, what can you say good of the climate of that island, where almost all the Europeans died? To which I reply that the mortality of those Europeans, though in some measure certainly to be attributed to the climate, was much more to the adventitious circumstances which have been already

noticed ; and I am inclined to think that, independent of its having really been the most unhealthy season of the year ; independent of our hard labour, and great exposure during that inclement season ; independent, I say, of all these, I am inclined to think, that much of our very great mortality may be attributed to *the uncommon depression of spirits* which our situation produced on the minds of most of our colonists ; and I verily believe that I should have died too, if I had ever suffered my mind to have been so subdued. But how far this despondency may have contributed to our mortality must be left to the decision of physicians.” p. 367.

Were I to hazard an opinion, I should be inclined to say that it contributed as a powerful predisposing cause to the action of infection, which already had accumulated in their bodies, like the electric fluid in the Leyden phial, and required only this excitement, to destroy at a single discharge—a review of all the circumstances of this ill-fated enterprize, appears to me to strengthen, perhaps confirm this opinion. Upon the whole this detail of them, extracted from the Journal of an eye witness, and the principal actor and mover in

the scenes described, is I think, sufficient to prove that infection existed, that disease was communicated by contagion, and that the mortality occasioned by it, was most extensive, before the departure of the Hankey from Bulama. I make use of the expression *eye-witness*, for Paiba, it appears, was absent from the 8th August to 6th October. Captain Beaver thus states that person's return from Sierra Leone, p. 143. The cutter "brought back Captain Paiba, one of the subscribers, with his wife and child, and a servant-boy, and *what was of infinitely more importance, two Papel Grumetas*" (slaves).

The fate of the unfortunate people who left Bulama in the Hankey on the 23rd November, 1792, is described by Captain Beaver from letters which he received from Captain Cox himself, or from the information of others who had seen or heard of her at Bissao, and it is ascertained more fully by the extract from the Hankey's log-book which he subjoins in the Appendix. The circumstances related to me by Mr. Paiba relative to the Hankey's sailing to Bissao on account of the unwholesomeness of the water

at Bulama, the seamen procured from Sierra Leone, &c. Essay, V. i. p. 108, do not correspond with Captain Beaver's narrative, and seem to be a jumble of unconnected facts which had occurred at different times. Captain Beaver, p. 173, thus states the situation of the Hankey from Captain Cox's own report. December 17, "Arrived two boats from Bissao, one of which brought letters for me from Captain Cox of the Hankey, which ship is now at that place, having run on shore near the isle of Formosa; his ship, he says, is exceedingly sickly, having buried since he left us, no less than six of his crew, and six Colonists, *which is somewhat astonishing, as there were only two of the crew slightly ill when he left this port; and not one had been buried during the whole of their stay here.*" Again, p. 192, "The Industry returned this evening, (12. Feb.)—Learned that the Hankey had arrived after a passage of only six days, at St. Iago, but in such a sickly state that *only five men were alive out of the passengers and crew.*" There is a note on this passage constructed for the purpose of disproving the information from Bissao, and subverting the opinion naturally resulting

from it, of the presence of pestilential infection, but the perfect correspondence between this information and the Hankey's log-book, removes all doubt. Before the ship ran on shore, December 4, and before extraordinary fatigue was undergone by the crew and passengers, two of the crew died, and three more were taken ill of fever. Three days after this accident, viz. on December 7th, the log-book states "all hands on board sick." In so short a space could fatigue produce an effect so universal, so deplorable? On the following day it is reported "all the people who came from Bissao in the pinnace taken ill;" and on the same day two more of the crew died. The truth is the fatigue the people in the pinnace were exposed to, served as a stimulus exciting the pestilential contagion, till then latent, or at least, not fatally active, into activity. Indeed it seems evident that the circumstance of the exemption of the seamen mentioned by Captain Beaver, prior to the Hankey's sailing from Bulama, can be accounted for only on the same principle, only by the same well known law of contagion. It does not appear from Captain Beaver's Journal that any part of these people

were employed in any laborious occupation, but such as might be incident to duty as the crew of a ship at anchor and in port; consequently their comparative predisposition was trifling. A very remarkable and similar exemption you will find stated in my Essay, V. ii. p. 70, 72. Had the crew of the Harmony been exposed afterwards to fatigue and contagion, a similar result no doubt would have taken place. That the crew of the Hankey did not however, escape sickness of some kind or other during her stay at Bulama, appears from Captain Beaver's reports of sickness, from two to six of them being stated as sick daily. In the course of your very extensive and very accurate observation, many instances no doubt have occurred of this lengthy latent state of contagion. The result of your observation on typhous contagion you have in the clearest manner stated in your letter to Dr. Percival, by which it appears "that the latent period of infection varies from a few days to two months." p. 68. It is probable however that there are instances in which the latent state of contagion (I confine myself to the pestilen-

tial or “yellow fever”) is of much longer duration. It is indeed wonderful how long it may remain so, until roused by an accidental stimulus. A very singular instance of this was communicated very lately to me by an officer of high military rank and respectability, in whose person it occurred. This gentleman had been frequently during the years 1794, 5, in different islands of the West Indies, where the malignant pestilential fever raged, exposed to its contagion, and even in contact with friends labouring under a fatal attack of it, without once perceiving in himself the least change of health, or tendency to the disease, except an unaccountable dinginess in the colour of his skin. At length at a gentleman’s house near Sandy point, St. Kitt’s, he happened to eat at dinner some poisonous fish, which during the following night produced the most excruciating convulsive affection of his stomach and bowels. This affection was removed by proper remedies; but three days after, it gave activity to the pestilential poison long since lurking in his system, and to a train of symptoms of the malignant pestilential fever of the most distressing nature, from which a

change of climate alone enabled him perfectly to recover*.

The extract from the Hankey's log-book is a very important document, it furnishes a convincing proof that there was a direful infection on board, and it proves too some points which Paiba flatly denied. Thus the Hankey is stated to have arrived at Grenada on the 19th of February, 1793, instead of the 19th of March; a supposed fact on which much of Dr. Smith's argument was founded. Thus too it is stated that on October 18th, 1793, at Stangate creek "*delivered up the Bulama baggage,*" which in the note, p. 192, is said to have been *there* sunk. "The Hankey was sent to Stangate creek to per-

* But the instance mentioned by Dr. Wittman, in his *Travels in Turkey*, p. 284, far exceeds this, and indeed is the most extraordinary fact of the kind I have ever met with: "A barber-surgeon at Cairo," says he, "at the very advanced age of 96, fell a victim to the plague of the year 1800. He had attained a high celebrity among the pestiferous patients, whom he had bled and attended from an early period of life. It was therefore extraordinary that he should have received the infection for the first time at so advanced an age, when it was reasonable to apprehend that from the torpor and inactivity which must have been generally induced in the temperament he would have been least susceptible to disease."

form quarantine, and orders were afterwards given for sinking the ship and her cargo; however on examination, the falsehood and malignity of this report," of her having carried the plague from Bulama to Grenada, "being proved, this order was confined to the Bulama baggage only;" who were the parties examined in this case? The parties whose interest it was to conceal the truth. Again we observe that no notice whatever is taken in the log-book of the *purifications* so sedulously resorted to according to Paiba's narrative, nor indeed is it likely in her deplorable situation, that the very few at all capable could attend to any such laborious preservative measures. The preservation of "the *Bulama baggage*" is a further proof of the duplicity, a harsher expression might be more applicable, of Captain Cox, who had assured the gentleman who had chartered her at Grenada, that this baggage had been destroyed at Grenville bay.

As much stress is laid on the impunity with which the Charon and Scorpion had intercourse with the Hankey at St. Iago, it may be proper to observe that this is readily

accounted for by Paiba himself, (Med. Rep. V. i. p. 480), where it is expressly said that the Charon's people were never permitted to leave the barge whilst it lay along-side for a few minutes morning and evening; as to the intercourse with the people of the island, and the entertainments given on board the Hankey, to them, they rest at best on meagre authority; and I think may be considered, in the state of that ship, highly improbable. Of the seamen furnished by the Charon and Scorpion, one sent from the former died on the 4th February, seven days after leaving St. Iago; another was put on shore at St. Vincent in a deplorable state, and died before any medical assistance could be procured for him. The Paiba who arrived at Grenada in the Hankey, was not a member of the council, but merely a subscriber or adventurer. Paiba of the council returned to England in the Calypso.

I have dwelt more particularly on this part of my subject, because I consider it of high importance to attend to the real state of this ship; for the circumstances which

preceded, compared with those which immediately followed her arrival at Grenada, of which an ample account is detailed in my Essay, V. i. p. 120, 138, clear up the mysterious nature of the fever, which was their decided offspring; and constitute a luminous confutation of the assertions of Dr. Miller and his honest informant Paiba. “All Dr. Chisholm’s leading assertions, concerning the pretended introduction of the Bulam fever into the West Indies are positively denied by Mr. Paiba, a gentleman of intelligence and unblemished character, *who was on board of the ship, charged with the importation, during the whole of the voyage.*” Med. Journal, July, p. 305. They will I imagine, enable physicians to discriminate with accuracy between the nature and effects of pestilential infection of adventitious, and those of morbid causes of endemic origin strictly so called, and attached inseparably to certain defined situations: they will enable them to trace with as little ambiguity as our imperfect knowledge of the operations of nature will permit, the sources of both, and the peculiar character of each: they, in

them; will perceive those points towards which these causes approximate, but in which they cannot meet:—they may see, in them, the line of distinction, between the *Febris Maligna pestilentialis*, and the “*Febris ardens biliosa*:”—and finally, by the series of causes and effects in them exhibited, the difficulties which obstructed the delineation of their doctrines, may be so far removed, as to render the perplexing, irrational, and unscientific combination of two irreconcilable causes operating one and the same effect, no longer admissible, no longer the reproach and disgrace of Pathologists, no longer the real source of the greatest danger necessarily attached to infection and pestilence. The compilers of the “*New London Medical Dictionary*,” published in the present year, seem to have formed a very just and correct conception of the nature of the malignant pestilential fever; and which, were it not for the adoption of the unmeaning name “*Febres ardens biliosa*,” as applied to it, would be precisely what it really is. The views which they have given of this ‘*destructive monster*,’ as they very appo-

sitely call it, "will discriminate it," say they, "from the causus, from the gaol and hospital, as well as from the common bilious fever. In gaol fever there is little accumulation and discharge of bile; in the others, little of the asthenic and putrefactive state. The yellow fever and the plague, are, we think, referable to the latter; and the distinction is not an object of curiosity and refinement only, for it assists in directing the cure, particularly the *exhibition of the Peruvian bark*." (I wish this however to be received with great caution for experience has made me a skeptic in this). They proceed—"much idle—it is an improper word—many highly pernicious disquisitions have been indulged, whether this fever is contagious. The existence of a doubt shews it may not be highly so. It has been doubted whether the plague is contagious; but those who have suggested and disseminated the doubts, are answerable for the lives of thousands, and in some instances have paid the forfeit with their own." P. 1. p. 246. I the more readily quote this opinion, as the compilers, in this instance at least, probably in all others,

seem to have a bias to neither party---and as they do not seem to be much inclined to favour me, if I may judge from this, and from the marked manner they appear to have neglected to attribute to me, what is certainly my right, the merit, if it has merit, of discovering, and first applying and promulgating the wonderful efficacy of mercury in this fever, a remedy which, nevertheless, I am happy to see they unequivocally approve of--p. 248.

To conclude, let the unbiassed reader now become umpire, and decide whether or not “a nova pestis---a peculiar, original, foreign pestilence, recently generated, and utterly unknown before, endued with a new and distinct character, possessing new powers of devastation, and capable of propagating itself by contagion throughout the world,” was introduced by the Hankey on the 19th February, 1793, into Grenada. The demonstration of my senses, and the clearest perception of my mind, assure me it was a nova pestis, to which the observation of Thucydides is truly applicable; for never before had so dreadful a pestilence occurred

in the West Indies, nor such destruction of the human race been recorded---ὁ μὲν τοι τοσσετος γε λοιμος, ἐδὲ φθορά ἔτως ἀνθρώπων εἰδαμὲ ἐμνημονευστο γενεσθαι. Indeed if we make a parallel between the two fevers, that of Grenada and the τοσσετος γε λοιμος of Athens, we shall find that they bear a strict affinity to each other in their causes and symptoms. They seem indeed to have been the same disease, the same “destructive monster.” If they differ in one symptom the large bilious discharges noticed in the Athenian, ἀποκαθαρσεις χολῆς, they agree in so many other circumstances as almost to establish their identity. There cannot indeed be a stronger instance of the exclusive power of preconceived opinion, of determined scepticism in medicine, than the tortuous application of Thucydides his description of the Athenian monster, to the support of the doctrine of the domestic origin of the fever of 1793, attempted by Dr. E. H. Smith of New York. Med. Rep. v. 1. p. 3.—And his attributing the violence of the disease to the deficiency of vegetable supplies; and the increase and decrease of the former to the proportions of the latter, is too fanciful, to

admit—is too repugnant to common sense, to appear even as a minute material in the fabric of septon. I shall here beg leave to arrest your attention to a few of the circumstances of the Athenian plague which remarkably verify the apparent identity of the two diseases. The peculiar febrile heat, and singular colour of the surface of the body, “*Corpus quidem exterius, tangenti non erat admodum calidum, neque pallidum, sed subrubrum, lividum, &c.* (καὶ το μὲν ἔξωθεν ἀπτομένῳ σῶμα, ἐκ ἄγαν θερμὸν ἦν, ἔτε χλωρὸν, ἀλλ’ ὑπέρυθρον, πελιδνόν, &c.)---the singular affection of the private parts---nam in ipsa quoque *pudenda*, et in *summas manus*, *summosque pedes* prorumpebat, (κατέσκηπτε γὰρ καὶ ἐς ἀκρας, χειρας, καὶ ποδας) where it is to be observed that the insertion of the comma after the word *ακρας* clears up all the ambiguity remarked by some fastidious commentators on this passage;---the *amentia* or peculiar insanity which patients in these fevers sometimes fall into, of which the case of the officer I have above alluded to is a remarkable instance---*nonnullos etiam, simul ac ex morbo convaluerunt, statim omnium rerum oblivio*

pariter cepit, &c. (τὲς δὲ καὶ λήθῃ ἐλάμβανε παραυτίκα ἀναστάντας τῶν πάντων ὁμοίως);--the infrequency of relapse or reinfection, “hic enim morbus eundem bis non corripiebat, ita ut eum etiam interimeret,” (δὲς γὰρ τὸν αὐτὸν, ὥστε καὶ κτείνειν, ἐκ ἐπελάμβανε);--its more violently afflicting strangers from the country, and such as were most laboriously employed, “ipsos autem, præter laborem, quo jam vexabantur, ipsa quoque rerum ex agris in urbem comportatio, præcipue vero rusticos, qui in eam confluerant, gravius pressit,” (ἐπίεσε δ’ αὐτὲς μᾶλλον πρὸς τῷ ὑπάρχοντι πόνῳ καὶ ἡ ζυγχομιδὴ ἐκ τῶν ἀγρῶν ἐς τὸ ἄστυ, καὶ ἔχ’ ἥσσον τὲς ἐπελθοντας.)---Another remarkable circumstance in which the close affinity is perceived, is the disease in both cases breaking out in the harbour or port where the shipping lay, and that too suddenly; “in Atheniensium porro civitatem, *derepente incidit*, ac primum quidem in Piræeo homines tentavit,” (ἐς δὲ τὴν Ἀθηναίων πόλιν ἐξαπναιίως ἐνέπεσε, καὶ τοπρῶτον ἐν τῷ Πειραιεῖ ἤψατο τῶν ἀνδρῶπων, &c.)--the excessive sense of internal heat “interiora vero ita flagrabant, ut neque tenuissimorum vestimentorum, aut lin-

teorum injectiones sustinerent, (τὰ δὲ ἐντὸς ἔτιος
ἐκαίετο, &c.)---the morbid strength “ corpus
etiam ipsum, quamdiu morbus vigebat, non
languebat: sed præter omnem hominum
opinionem cruciatui resistebat,” (καὶ τὸ σῶμα,
ὅσον περχρόνον καὶ ἡ νοσος ἀκμαζοι, ἔκ ἐμαραίνεται,
&c.) There are many other points of coin-
cidence; but the foregoing are sufficient.
Dr. Smith in his Essay on this plague, says
the citadel does not appear to have been vi-
sited by it; and on this he no doubt founded
a very important part of his argument; for
if that had actually been a fact, the conta-
gious nature of the disease would have been
at least rendered problematical. But this
I apprehend is not the fact, for the words
of Thucydides go to establish the extension
of the disease to the upper city, in which
must have been included the citadel, for
that and every other unoccupied place, in
times of tranquillity, became the abode of the
inhabitants of the country in those of war,
“ deinde vero in superiorem urbem pro-
cessit et longe plures tunc moriebantur,”
(ὕστερον δὲ καὶ ἐς τὴν ἄνω πόλιν ἀφικετο, καὶ

ἔθνησιν πολλῶ μᾶλλον ἤδη.) * The extravagant exaggeration employed by Dr. Smith in his description of Athens is manifest to those at all acquainted with its ancient state; and to be assured of his excessive inclination to force every thing to a subserviency to his theory, it is only necessary to compare his account with that of the Abbe Barthelemi drawn from the best authorities, and chiefly from Thucydides, for its state at the period which his history comprises, and of which a contemporary, Lysippus the statuary, said “Whoever does not desire to see Athens is stupid; whoever sees it without being delighted is still more stupid; but the height of stupidity is to see it, to admire it, and to leave it.” Travels of Anarcharsis, v. 1, p. 307, 326, from Dicæarchi Stat, Græcæ.—That the disease was of domestic origin, is extremely doubtful. The only authority for the supposition is what Thucydides says of the immunity of the Pelopon-

* Smith contradicts himself (Med. Rep. v. 1, p. 15,) “the plague, about the last of April or beginning of May, suddenly appeared in the Piræus; and afterwards, during the summer, extended over the whole city.”

nese, during the evils which Athens laboured under, from the plague, “nec ipsam Peloponnesum invasit, nec ullo malo eam afficit, quod sit relatu dignum,” (καὶ ἔς μὲν Πελοπόννησον ἐκ εσσηλθεν, ὃ, τι καὶ ἄξιον εἰπεῖν.) But besides this indefinite mode of expression, and besides the existence of causes which might have produced this exemption in the Peloponnesus, a country nearly insulated, and with which the Athenians seem to have had no intercourse after the appearance of the plague in their city, there is manifest internal evidence of the presence of contagion; there is also the express declaration of Thucydides himself. From the manner in which he tells of its prevalence even in Greece before its invasion of Athens, it is almost certain he firmly believed it, and knew it to have prevailed; “quanquam et ante cum in multas regiones invassisse ferebatur, tum etiam in Lemnum et alia loca, veruntamen neque tanta pestilentia, neque tot hominum interitus uspiam accidisse memorabatur,” (λεγόμενον μὲν καὶ πρότερον πολλαχόσε ἐγκατασκήψαι, καὶ περὶ Δῆμον, καὶ ἐν ἄλλαις χωρίοις, &c.) He is more explicit af-

terwards, when he speaks of the divergence of the disease from the focus of infection, Athens, “*depastus vero est Athenas præcipue, deinde ex aliis locis ea, quæ maxima populi frequentia erant referta;*” (ἐπενείματο δὲ Ἀθήνας μὲν μαλιστα, ἔπειτα δὲ καὶ τῶν ἄλλων χωρίων τα πολυανθρωποτατα.) Can a doubt remain after so clear and distinct a deduction of the pestilence from Athens, where the concourse being greatest, it committed its greatest ravages, to other places, always directing its progress to those places chiefly where the assemblage of victims was greatest. These testimonies of Thucydides set aside the assertion of Smith, that the plague affected the Athenians only; and when strengthened by the unequivocal fact recorded by Plutarch, remove all doubt of its having been a foreign disease, imported by some of the Athenian trading vessels into the Piræus, and afterwards propagated by contagion, the malignity of which was augmented to an extraordinary degree by adventitious circumstances. The fact recorded by Plutarch is perfectly to the point. Pericles “laid siege to the sacred city of Epidaurus,

and at first with some rational hopes of success; but the distemper which prevailed in his army, broke all his measures; for it not only carried off his own men, but all that had intercourse with them;" (*πολίορκήσας τε την ἱερὰν Ἐπίδαυρον, ἐλπίδα παρυσχῆσαν ὡς αλωσομένην, ἀπετυχε διὰ την νόσον. ἐπιγενομένη γὰρ ἐκ αὐτῆς μόνον, ἀλλὰ καὶ τὰς ὁπώσῃν τῇ στρατιᾷ συμμίζαντας προσδιέφθειρεν.*—*tum obsidentem sacram Epidaurum, quæ spem præbuit capi se posse, morbus distinxit. Hic enim non ipsos solum, sed omnes quacunque causa ad exercitum accidentes affecit afflixitque.*)---
 Plutarch. Chæron. Vitæ Parallelæ Gr. & Lat. Ed. Bryani. Lond. 1729. tom. 1, 378.
 This passage of Plutarch is in truth a luminous commentary on the last quoted from Thucydides. The history of Pericles himself, and of his family, furnishes a very satisfactory illustration of the contagious nature of this dreadful disease; and also of that resistance to infection, or at least to its action on the system, which he remarkably possessed, and of which I have made mention in another part of this letter. He wit-

nessed the loss of all his family, and the greatest part of his relations and friends, and yet he continued unaffected by the virus to which he was continually exposed, until the autumn of the second year of the pestilence, when, as in the case I have already stated of an officer of rank in the West Indies, he was seized with the disease, a marked effect of which on him, was derangement of intellect. The stimulus which roused the latent contagion into activity and fatal effect in Pericles, was the misfortunes in his family, particularly the death of his favourite son Paralus, and the disappointment he experienced in his political views, by both of which, as Plutarch says, "he seemed to have suffered the punishment of his arrogance and pride." In quoting the foregoing passages from Thucydides I have made use of the Etonian Version.

These observations on Dr. E. H. Smith's Essay on the Plague of Athens, may seem to have little or no connection with the subject in discussion, but in fact they are important,

in as much as they tend to shew how fallacious the grounds are on which these theorists found their arguments, and consequently how visionary the conclusions they wish to deduce from them.

PROPOSITION IV.

That in my letter published in a mutilated form in the Medical Repository of New York, and quoted by Dr. Miller, I have not relinquished the doctrine and opinions I upheld in my Essay.

THE discussion of the last proposition, prepares for that of the fourth, viz. that in my letter published in a mutilated form in the Medical Repository of New York, and quoted by Dr. Miller, I have not relinquished the doctrine and opinions I upheld in my Essay:—and indeed as the one is involved in the other, that is, as they both depend on, and result from the same physical principles, the same laws of infection and contagion, so they mutually tend to the evidence and establishment of each other. Do we perceive inconsistency in assigning similar effects to

similar causes? is it not the uniform course of nature in all her operations? If I am answered in the affirmative, and the appeal is made to common sense,* as well as to philosophy, Dr. Miller must confess he has uttered the truth, whilst he conceived he proved my abandonment of the doctrine and opinion I published in 1794; for in truth, the facts I am about to state to you, “form a luminous and instructive commentary on my former opinion.” But let me be more explicit—I have already adverted to the inconsistency and absurdity of some of Dr. Miller’s dogmas, relative to the inapplicability of the word “Importation” to a disease generated on board a ship during her voyage; and one received by infection on board at the port from which she has last sailed.

* “The term Common Sense hath in modern times been used by philosophers, both French and English, to signify that power of the mind which perceives truth, or commands belief, not by progressive argumentation, but by an instantaneous, instinctive and irresistible impulse; derived neither from education nor from habit, but from nature; acting independently on our will, whenever its object is presented, according to an established law,” &c. Beattie’s *Essay on Truth*, Part 1, ch. 1.

Surely in relation to the port for which the ship is bound, the distinction is purely visionary. In the appendix to his report on the fever of New York of 1805, he promulgates this notion truly singular in its kind, if indeed any meaning can be attached to it. It seems to be a deduction from the consideration of garbled extracts from a private letter of mine to an ingenious medical friend, compared with the case of the Hankey. But I imagine it must require more ingenuity and more penetration than even Dr. Miller possesses, to discover in my account of the origin of the malignant pestilential fever, that I had any intention to derive it from any other cause but the wretched state of the people on board that ship. A little calm attention to the introductory part of the chapter of my Essay on the origin of the fever, will prove the injustice of Dr. Miller's charge---“ In ships therefore, in which the captains are not urged by motives of interest, to prevent infection; fevers of a malignant and pestilential nature, may be generated even in hot climates. A ship of this description introduced the very

fatal fever which raged in the town and port of St. George's, during several months of the year 1793"—and afterwards, p. 120, "from the period at which the Hankey arrived at Grenada, viz. 18th February, 1793, are we then to date the commencement of a disease, *seldom before known in the West Indies*, and certainly unequalled in its destructive nature." If there are no records of pestilence in the West India Islands but those of La Bat and Dr. Warren in the early part of the 18th century, there can be no impropriety in applying to that of 1793 the "*nova pestis adest*" of the poet, which seems to have more particularly caught the view of Dr. Miller. "The unknown singularity of the African atmosphere" must have originated in his own profound research:—is the offspring of his own wayward imagination. Having, as he fondly imagines, overthrown the fact of imported infection into the town and port of St. George's, he next attempts to treat in the same summary and cavalier manner, the diffusion of disease by contagion. But advocates of much greater ability than I have any pretension to, shall answer his vague

but captious objection. “ It has, however, been objected,” observe the learned and humane Institutors of the House of Recovery of Dublin, “ in the wildness of speculation, which has of late years so frequently led to the calling in question of established opinions, that as persons in the same family are placed in the same circumstances with regard to diet, air, exposure to effluvia, and general habits of life, fevers may arise from the limited operation of these causes, tending to generate disease, independently of contagion. But such objections when examined serve only to establish more firmly the contrary opinion. For if it shall appear, first, that when the above supposed causes of fever are in full operation, that still the disease proceeds as if it were propagated by contagion; and, secondly, that where no cause but contagion can be supposed to operate, still fever spreads amongst those exposed to it; and lastly, that the speedy removal of the sick from the healthy, will occasion a reduction in the number of fever patients to be found within a limited district in a certain time, all such objections must at once vanish.” See

First Report of the House of Recovery, Dublin, 1806.—The case of the Hankey and the Marie Anne, in Demerary, is precisely similar, except in the circumstance of climate, to that of the foul houses of the labouring poor in large cities. The accumulation of filth of every description, more particularly of putrid animal matter in a very limited and unventilated space, is the cause of the pestiferous atmosphere which the wretched inhabitants respire; a pestilential fever is the consequence, and this fever is propagated by contagion, or the aura which emanates from the persons, and the infectious matter which attaches itself to the clothes, or bedding, or furniture of the sick, or of the chambers where the sick are lodged. The learned, the ingenious, and truly benevolent physicians, at the head of whom stand yourself, who have reported on the state of the habitations of the labouring poor of Chester, Manchester, Liverpool, Newcastle-upon-Tyne, and lately of Dublin, furnish most ample illustrations of this most important fact in medical physics, that the vapour or exhalation arising from animal

matter accumulated in a putrid state, and rendered stationary by the neglect of ventilation, is universally the cause of the fever of infection, the typhus, which annually diminishes the population of these cities. Their ingenious and humane exertions have most clearly manifested the remedy: remove the accumulated putrefaction, produce perfect ventilation, decompose the matter of infection, or purify the pestiferous fomites contained in the houses or chambers of the sick, and enforce this improvement or purification by those stimuli which alone can act on unenlightened minds, pecuniary rewards, and you give health and comfort to those who have been bereft of both. The same causes probably give origin to the plague: and, in the case before us, the produce of such a state of circumstances is marked with a peculiar character, by the climate in which it takes place. In cold and temperate climates we see a *simple* fever of infection, or typhus, is the consequence; within the tropics, there is superadded to the most malignant virus of typhous infection, many of the distinguishing features of the most violent yellow

remittent fever, thus forming a monstrous *compound*, which hitherto has not found a place in any nosological arrangement. It is a disease which, from this peculiar conformation, is defined with difficulty, and is distinguished by shades which require the industry, the discernment, and the fidelity of a Claude Lorrain to delineate. It is hence we so often meet with counterfeits, and with so many on whom the imposture has been too successfully practised; and it is hence a “*Libro di Verità*” of its history and portraiture is become so indispensibly necessary. It is a disease, however, more obvious to the senses of the experienced, discerning, and unprejudiced observer, than capable of being thoroughly conceived by description. The coup d’œil of the former at once perceives and distinguishes it; in the latter it becomes too often doubtful, too often assumes the garb of the malignant offspring of marshes, when, in truth, it is “hell-born”—it is

the other shape,
 If shape it might be called that shape had none,
 Distinguishable in member, joint or limb,

Or substance might be called, that shadow seem'd,
 For each seem'd either; black it stood as night,
 Fierce as ten furies, terrible as hell,
 And shook a dreadful dart; —————

PARAD. LOST, B. 2. 666.

It is wonderful, it is almost beyond belief, that ingenious and well-informed men, (I mean Drs. Mitchell and Miller, of New York), should endeavour to impress on the minds of their readers, that I have rescinded this opinion; that they should artfully insinuate, by a very singular perversion of a very clear and unelaborate statement of facts, that I have coalesced with themselves. The fact is, that the fever generated on board the ship *Marie Anne*, in her voyage from Liverpool to Surinam and thence to Demerary, was absolutely infectious; but its contagion, *sublimed* by climate and uniting probably with the marsh miasms which abounded at the time in the neglected trenches and canals of Stabroëk, produced a fever of a mixed type, “the destructive monster” of the Hankey. It is evident that this has been uniformly my opinion since 1793; so that when Drs. Mitchell and Miller concede the

the fever of North America since that year, has not been uniformly endemic, a prospect will then be held out of union and coalescence of sentiment; but at present our opinions are as two lines which approximate but cannot meet: what these ingenious gentlemen are to yield, is what they now profess to dissent from. “ But in two points we dissent from Dr. Chisholm: first, in his opinion of the *peculiar nature* of the pestilential disease which appeared in Grenada and Demerary, and of its differing essentially from the yellow fever as it prevails in the West Indies and on the American continent. And, secondly, in his opinion of the contagiousness of the pestilential disease generated on board of ships, while he justly acknowledges that the yellow fever is not capable of being propagated by such means.” *Med. Repository*, v. 5, p. 233. I am rejoiced to see that they have manifested an inclination to this necessary concession,—a concession which is in truth the sober dictate of common sense,—a concession which would do equal honour to their judgment and their humanity,—a concession which would remove all obstacles to personal

and commercial safety,—a concession which every circumstance of the case most obviously requires. Ibid. p. 231, 233. The compound which results from the state of circumstances these gentlemen detail, p. 233, I have above adverted to, and they have virtually admitted, although they endeavour to evade the effect of this admission, by a concluding dissentient remark: “As to the mixed or hybrid forms of malignant fever, concerning which Dr. Chisholm offers his conjectures, we can only observe, that they appear to us to be founded upon an analogy too remote and fanciful to serve as the basis of sound and correct reasoning.” This may be considered as the still smoking but nearly extinguished embers of controversy; and, for the benefit of mankind, may its obsequies be duly solemnized—

“ Postquam collapsi cineres, et flamma quievit,
Reliquias vino et bibulam lâvere favillam:
Ossaue lecta cado textit Chorinceus aheno.
Idem ter socios purâ circumtulit undâ,
Spargens rore levi et ramo *felicis olivæ*:
Lustravitque viros, dixitque novissima verba.

VIRG. Æ. 6, 226.

It would have been well that Dr. Mitchell and Dr. Miller had adverted to a fact which I communicated to my correspondent in the letter which contains the subject of our present discussion. They would probably join me in execrating the authors of the calamity; in saying with me that "Captain Cox, of the *Hankey*, and Captain M'Nab, of the *Marie Anne*, merit the most exemplary punishments."—"Aug. 30th. Having had no opportunity by which I could forward this letter, I shall fill the remainder of the sheet, with an account of a remarkable instance of infection on this coast, (east sea coast of Demerary). A young man of the name of Allison arrived only in June last from England, his native country, was unfortunately exposed in town to the contagion of the fatal fever, which I have given a general history of. His brother, anxious to remove him from a pestilential air, went to town; and, although the young man was already labouring under the incipient symptoms of the pestilential remittent, yet he insisted on his proceeding up the east coast. On the 26th or 27th of July they stopped for the night

at Lusignan, the plantation of K. F. Mackenzie, Esq. three miles further from town than Success, and consequently eleven altogether. They put up with the manager, Mr. Reid, who, from mere humanity, gave up his hammoc to the sick man. On the following morning, the two brothers continued their journey to windward, and that evening the sick man expired. The manager of Lusignan, having no suspicion of the nature of Allison's fever, took no precaution to destroy the seminia of infection retained by the hammoc, but made use of it as he had been wont to do. An overseer and two white carpenters, natives of Scotland, lodged in the same house. The former and one of the latter, being probably more disposed to be acted on by the infection, were the first who suffered by it: the overseer died on the seventh day, the carpenter recovered with difficulty. Mr. Reid was not affected for a considerable time; at length, about the middle of this month (August), he fatigued himself in the course of his duty, and instantly a predisposition was formed. This is an instance of accumulation of morbid

agency, inactive till put in motion by a stimulus, like the electric spark. Similar instances were frequently observed both at Grenada and Philadelphia during the presence of pestilence at these places. On the evening of the 23rd, he was attacked with a fever, the description of which seems, from Mr. Mackenzie's and Dr. Ord's statement, to have been for two days remittent. On the third day, however, its type, and the aspect of the patient, became altogether different, and decidedly assumed those of malignant pestilential fever. The day after this, the fourth, Mr. Mackenzie, justly alarmed, entreated me to see the patient, and the other carpenter who had been seized in a similar manner on the 24th. You may believe nothing effectual could be now done. Mr. Reid died on the fifth day, and the carpenter also on the fifth day of his fever. Here then of four people, all of whom had been two or three years in the country, three died. I begged Mr. Mackenzie to employ every possible means to prevent the spreading of the evil from the manager's house, by burning every thing suspected of retaining infection, by

fumigating with the nitric acid gas, and by white-washing. These directions I trust will be attended to, and have the desired effect." This was the P.S. to my letter to Dr. Davidson, which either was not communicated to the editors of the Med. Repository, or has been suppressed by them. It cannot, I apprehend, be denied, however, that it is an instance of disease proceeding immediately from contagion "*the vitiated product of living vascular action,*" and mediately from infection "*the venomous offspring of putrefaction:*" and we consequently see in it these two causes so subtilly distinguished, Med. Rep. v. 5, p. 186, acting, as they certainly always do, in succession, that is, as cause and effect, in the production of pestilential and typhous diseases. With small-pox and other specific contagions, as they are called, which are referred to "*vitiated living vascular action*" exclusively, although, observing the accustomed and undeviating course of nature, no doubt, originally the product of a peculiar infection, we have here nothing to do.

What became of the Allison who left Lushignan well, I have not heard; but, notwithstanding the adoption of the preventive measures I recommended, the infection unhappily continued to lurk about the buildings of the plantation, where it had already committed such havoc, and, by means of contaminated linen, I have understood, it sometime after was conveyed to the dwelling-house, and there seizing the children of my excellent and accomplished friend, proved destructive to every one of them, three in number, then in the house. Mr. Mackenzie himself escaped by having been subjected to an important law of the malignant pestilential fever, the contagion not possessing the power of acting a second time on the same person. But the ravages this dreadful monster committed in the shipping, and among the inhabitants of Stabroëk, were truly terrible and melancholy. Ships were entirely stript of their crews, houses were deprived of almost the whole of their inhabitants. But it evidently bore heaviest on the unassimilated to the climate, the newcomers from Europe. One instance I more

particularly recollect: a brig from Glasgow, having fourteen young men passengers; of these, twelve were swept away soon after being exposed to the baneful effluvia of the disease, with which the furniture, clothes, and walls of almost every house in the town, more especially those possessed by merchants and others connected with commerce, were infected. At this fatal period, I have reason to be assured, that not a single young man immediately from Europe, escaped the infection, unless taken into the country on his arrival before he could be put in the way of danger, by entering any of the houses of Stabroëk. For the general state of the coast of Guiana, with respect to disease, before the year 1800, I beg leave to refer you to the second edition of my Essay, v. 2, p. 189. Lusignan before the fatal visit of Allison, was considered and found, one of the most healthy and pleasant spots on that most healthy part of the West Indies, the east sea coast of Demerary.

HAVING now gone through what I had chiefly to offer towards the establishment of my four propositions, I can only add that I perceive from the facts I have stated, a moral certainty of the truth of these propositions. Nay I am inclined to believe that every reasonable unprejudiced mind will have a similar perception from the attentive consideration of them; of facts not observed and arranged by myself, which in that case might be subject to partial, ambiguous, and inconclusive inferences, but of other men of high respectability and worth, eye-witnesses and actors in the scenes they describe. “When a number of persons,” says Dr. Beattie, “not acting in concert, having no interest to disguise the truth, and sufficient judges of that to which they bear testimony, concur in making the same report, it would be accounted madness not to believe them. Nay, when a number of witnesses, separately examined, and having had no opportunity to concert a plan beforehand, do all agree in their declarations, we make no scruple of

yielding full faith to their testimony, even though we have no evidence of their honesty or skill; nay, though they be notorious both for knavery and folly; because the fictions of the human mind being infinite, it is impossible that each of these witnesses should, by mere accident, devise the very same circumstance; if therefore their declarations concur, this is a certain proof, that there is no fiction in the case, and that they all speak from real experience and knowledge." (Essay on Truth, p. 1, c. 2, s. 8). If therefore the testimony I have adduced is unworthy of belief, it follows that the authors of it are dishonest men acting in concert to deceive; but they are universally known to be honest men, and it is evident that within the limits of moral possibility they could not have acted in concert—consequently their testimony amounts to the demonstration of our own senses, and must be followed by that conviction which is produced by the evidence of sense. This is a syllogism of which I have the fullest conviction, and which I am confident you will give the most ample credit to. The conclusion is irresistible. It is in my

*yes! & the ending I in places
 of the text is "plague"
 finalism possible from
 the whole of the text*

mind a truth as evident as my existence. Such is the impression on my mind, resulting from it, and I trust the same impression has been made on yours, and on all others, not determinedly sceptic, as that which must proceed from those instinctive suggestions which are the foundation of all truth. When the sceptic can prove that every thing in nature exists necessarily independently and from eternity, then shall I yield up my conviction of the truth of my propositions.

I imagine it would be doing injustice to my subject, did I not advert to a very striking peculiarity of the malignant pestilential fever; I mean its affinity to that disease, which, to be distinguished, has been named the *true plague*. I know however there are some excellent men, and learned and enlightened physicians, who have more than insinuated that, in proving this affinity, too much is proved; or in other words, that he who attempts to establish that the *true plague* and the malignant pestilential fever are one and the same disease, goes far to overthrow the doctrine of infection and contagion

in general. There is nevertheless a manifest distinction; for whilst it is evident even to the *common* observer that an affinity between the two diseases exists, it is evident to the *acute* observer that there is a line of demarkation. Thus an affinity is manifest in the remote cause, infection, and in the predisposing habit of body necessary to give activity to the virus of that infection: thus there is a distinction in the mode of divergence of the effluvia emanating from the diseased to the healthy; and in as much as the same atmospheric temperature promotes in one and checks in the other, the diffusion of contagion. This is the cause why the two diseases have never been found to exist at the same time in the same country; this is the cause why *true plague* has never been seen within the tropics; it is also the cause why the malignant pestilential fever may exist without as well as within the tropics, when the temperature of the atmosphere of the former rises to the degree generally prevalent in the latter. It may be said that this is theory, or a deduction from premises not proved, and not the result of experience. It

is not indeed altogether the result of my own experience, for I have never been so circumstanced as to observe the true plague; but it is the result of the experience of a very acute, and I have every reason to believe, a very unprejudiced observer. In this instance the remarks made are more deserving our notice, as they were made by a physician who had no knowledge of myself personally, and was not aware of this affinity, at the time he witnessed the true plague, although that time was subsequent to his having witnessed the malignant pestilential fever in the West India Islands. Dr. M'Gregor, the very intelligent and respectable author of "Medical Sketches of the expedition to Egypt," served in the West Indies as surgeon to the 88th regiment during the year 1795 and part of 1796, consequently during the prevalence of the malignant pestilential fever, and more especially during its very destructive prevalence in Grenada in the former year. He afterwards served in Egypt as superintending surgeon to the Indian army acting there. The Sketches constitute a manifest proof how accurately he observed, and how judiciously he applied

his observations. The author gives, without comment, what he himself or his medical brethren of the army had observed. The journal is judicious, and leads the reader from fact to fact without embarrassment, and with considerable elucidation of the causes of diseases chiefly prevailing. In short, that the author is disinterested and has the public good for his object, appears evident from the manner and matter of his narrative. Among the various objects of public utility they tend to establish, this is a very prominent one, that a clear distinction is marked between typhus and “yellow fever,” on one hand, and “yellow fever” and plague on the other: thus, from actual experience, giving that gradation of infection which these diseases observe, and thus placing “yellow fever” in that middle state which is natural to it, and impressing on it that mixed, that monstrous character it is certainly possessed of.

I have remarked, that the malignant pestilential fever possesses an aptitude to acquire such symptoms as may arise from peculiarity of situation: its affinity in this respect to

plague is evinced in the following instances. First, The plague which broke out in the crowded hospitals of the 61st and 88th regiments assumed the typhoid or hospital fever form ;— Sketches, p. 111. Secondly, When it appeared in the encampment near El Hammed in marshy ground, it assumed the remittent and even intermittent type ;— Ibid. and p. 124. Compare this with the malignant pestilential fever which prevailed in Demerary. See also Dr. Wittman's Travels in Turkey, where the same fact has been observed, p. 418. This is a circumstance which may reconcile the apparent differences perceived between the Philadelphia and Grenada fevers by Dr. Rush. See Essay, v. i, p. 235. Thirdly, When the plague appeared in comparatively cold weather, i. e. in a temperature varying between forty-nine and seventy degrees, and moist with rain, it assumed much of the inflammatory diathesis ;—Sketches, p. 112. Fourthly, When it appeared in places where there were no marshes, but a free circulation of uncontaminated air, the temperature of which was between sixty and ninety degrees, and

under circumstances rather exhilarating than otherwise, it assumed the type and form of a mild continued fever. This instance, however, may be considered as constituting one of the points of demarkation between the two diseases. Fifthly, The latent period of infection seems to vary as much in plague as in the malignant pestilential fever;—p. 121—123. Sixthly, Cases of plague have occurred in which the action of contagion seems to have been unattended by fever;—p. 124. The affinity in this particular has been remarked;—Essay, v. i, p. 321. Seventhly, In the plague it has been observed that the glands about the neck have been so much swelled that the patient has died of suffocation. This has also been remarked by Dr. Russel; but I mention it here, as a remark proceeding from a physician who has also seen the malignant pestilential fever, in which the same termination from the same cause has taken place;—Essay, v. i, p. 178. Eighthly, The short duration of life under the action of pestilential virus is another mark of affinity;—Sketches, p. 130, 131;—Essay, v. i, p. 194. Ninth-

ly, Another circumstance in which the affinity is very remarkable, is the gradation which the plague observes in its attack of those exposed to its infection, which I believe Dr. M'Gregor has not noticed, but which Dr. Wittman has particularly stated, (Travels in Turkey, p. 523), and which is almost precisely similar to the statement I have given of the gradation of the malignant pestilential fever, Essay, v. i, p. 140. As salini states the same fact, although he is inclined to make a different use of it;---Observ. p. 17, 18, &c. Finally, Dr. M'Gregor observes, "in general it was found that the patient recovered in proportion to the facility with which his gums and skin could be affected," p. 133---140. This I need scarcely observe was a most prominent feature of the malignant pestilential fever. This remarkable point of affinity is most particularly deserving notice; and it is to be earnestly hoped and wished that the medical gentlemen who may in future have the charge of pestilential patients, may avail themselves of the very important notices Dr. M'Gregor has given on the mercurial treatment of the

plague. With this remedy, and possessed of intrepidity, promptitude, and firmness of mind proportioned to the imminence of danger, they may in a very considerable degree prevent the usual destructive consequences of plague. The table which Dr. M'Gregor has constructed furnishes a very ample specification of points of affinity, and for further information I beg leave to refer you to it.

Let us now proceed to examine those points in which a manifest distinction has been observed. First, The difference between plague and the malignant pestilential fever appears chiefly in the mode of communicating the contagion. In the plague Dr. M'Gregor thus states it: "*The thirteen gentlemen first mentioned were those only that were directly in the way of contagion, for it became their duty to come in contact with the infected, and seven of them caught the infection and died. To the atmosphere of the disease all the medical gentlemen of the army were exposed, as they saw and examined the cases in the first instance: but, except from actual contact there never ap-*

peared to be any danger,"—p. 108. It is therefore, and the fact is further confirmed by some observations of Dr. Russel's, pretty evident, that contact is necessary to communicate the disease. In the malignant pestilential fever, around the diseased person there is an infectious atmosphere of a determined radius, within which the disease may be communicated: in it contact is not a necessary condition. It is from this discriminative circumstance, we see the latter disease so readily and generally diffused, and with so much difficulty checked or eradicated; and it is this which enabled Dr. M'Gregor to enjoy the pleasing sensations which a conviction of the truth of the following declaration must have excited. "If in the treatment of the disease we were not successful, we assuredly were completely so in the prevention;"—p. 142. I am inclined to think that M. Assalini and the other French medical men who served in Egypt, have been led to form their novel opinion respecting the plague, from the necessity there seems to be for contact in communicating its contagion. Thus if a person labouring under

plague should lie or be placed *near* another in health, but *far enough* to prevent contact of any kind, and should the latter continue well, the natural result in an observer's mind would be that the plague is not contagious. Thus probably Assalini observed and concluded. This remark militates against your opinion, which indeed is the prevalent opinion among physicians, "that the plague is never caught, except by contact, is an erroneous notion;" (Letter to Dr. Percival, p. 59); and indeed I should not have admitted it, had it not been warranted by so clear and distinct a proof; a proof not subject to the suspicion of deception, but the result of experience. In this view the malignant pestilential fever is brought nearer to the character of typhus. Secondly, A second distinction is perceived in the atmospheric temperature necessary to give prevalence to each disease respectively. This is a very curious circumstance, and certainly in many respects a very important one. The contagion of plague can become prevalent only in a temperature between forty and eighty degrees, a temperature below or above these

degrees extinguishes it: the contagion of the malignant pestilential fever becomes prevalent only in a temperature between seventy and ninety degrees; but it is extinguished or suspended in a higher, and probably cannot exist in a much lower. This you will recollect is not inconsistent with the observations I have offered on the subject in my Essay, p. 1, c. 9, although I confess at the time I made these observations, I applied them differently. Subsequent reflection has made the point more clear*.

I entertain the hope that what I have advanced in the preceding sheets, will fully bring to light the pernicious tendency of that doctrine, which, in direct contradiction to the general belief and experience of all men in all ages, goes to the non-existence of infection and contagion as the cause of pestilential fevers, more especially of “yellow

* The observation of Mertens, that the plague was not extinguished at Moscow by the keen frosts of the winter of 1770, may be an exception; and may arise from peculiar causes, particularly the close confinement of families in heated rooms.

fever" so called, (malignant pestilential), and to the subversion of the means. which the same experience has ascertained to be preventive of their fatal consequences. I mean, however, to proceed a little further, and by an examination of Dr. Miller's "Report" endeavour more fully to prove it to be a "mystery of falsehood," arising from the ambiguity of its language, and from the gratuitous admission of principles which never can be admitted when thoroughly understood. If it is believed that infection is an idle idea or fancy, existing only in the mind of weak, credulous, or superstitious men, or of physicians whose knowledge is merely prescriptive---if it is believed that the effluvia proceeding from a body suffering under the effects of a pestilential disease, have no power to produce the same disease in a healthy body placed within its influence?---let me ask, where shall we look for safety in seasons of pestilence?---Like sin and vice, without the restraints of religion and morality, infection and contagion will become universally diffused, and universally destructive of the human race. When then

an ingenious and learned physician comes forward with such a purpose---with a pre-determination to break down the barriers which alone can secure mankind against the inroads of these calamities---in what light are they to view him?---as Uriel viewed Satan. I feel it therefore my duty, and it is the duty of every one who respects the best interests of mankind, to resist him. Mankind are not thus to be betrayed, thus cajoled out of their senses. The book of wisdom is open to them---let them therein read, and adopt the counsel of the son of Sirach: "Let the counsel of thine own heart stand: for there is no man more faithful unto thee than it---for a man's mind is sometimes wont to tell him more than *seven watchmen that sit above in an high tower.*"

EXAMINATION
OF
DR. MILLER'S REPORT.

AFTER stating, in a very impressive manner, the embarrassments of the American commerce in foreign states, and declaring what no person will feel inclined to dispute, that "nothing but a thorough investigation of the subject, and the adoption of a wise and mature system of measures will be sufficient to ascertain and set in operation any adequate means of relief," (*Edin. Med. and Surg. Journal*, July 1807, p. 277), Dr. Miller proceeds to describe (unintentionally no doubt) the usual progress of an infectious fever in a pestilential season; first, sporadically when the infection has been first received; and then epidemically, when the

contagion has been generally diffused; and at length its total cessation, when the temperature of the atmosphere is insufficient to propagate and give it its peculiar activity in the case of "yellow fever." I trust I have rendered it sufficiently evident, in the discussion of my fourth proposition, that the cause of the fever of New York in 1805, was imported infection: if this required further evidence, I imagine Dr. Miller himself furnishes it. "The source of this disease," says he, (p. 278), "forms a most interesting subject of inquiry, on the success of which must depend all rational and adequate means of preventing and eradicating the evil." This is indisputable; but the discovery of this source does not depend on the misapplication of language, nor does it require the aid of such a sophism as the following: "After a long and careful investigation of the subject, I cannot hesitate to conclude," says he, (*ibid.*), "that a pernicious exhalation or vapour, floating in the atmosphere, is the primary and essential cause of this disease: to produce this there must be a concurrence of heat, moisture, and a quan-

tity of decaying animal and vegetable substances. It is more frequently and copiously produced and more highly concentrated in warm and tropical countries, than in high latitudes and frozen regions; it is generated more in summer and autumn; and is uniformly more frequent and virulent in seaport towns, in situations along the sea-coast, &c. than in the interior high and mountainous districts of a country. It is undoubtedly one of the most universal causes of disease in nature." Here we observe an artful mixture of truth and falsehood; for although the induction is applicable to marshy countries, the temperature of whose atmosphere is tropical, yet it remains to be proved that New York is a city situated amidst marshes, or that at any period since 1793, it was so. It remains also to be proved that this city has been so defective in its police, as to contain, as a cause, after twelve years' experience of a pestilential effect, "low and moist grounds, overspread with the corrupting offals of animal and vegetable substances, collected in large masses in which the process of putrefaction is going on to considerable

extent.” This part of the subject I have already amply discussed, and have shewn the indetermination, the wavering, evasive, and fugitive opinion of Dr. Miller’s predecessors in the field of sophistry; it may therefore be sufficient to add here the very apposite remark of Mr. Tytler—“ The dilemma stands thus: if the yellow fever is produced by the effluvia of marshes by putrid steams, or by any thing else (local), how comes it to pass that it has been so frequent in the United States since the year 1793, in comparison of what it was for thirty years before. Have the American cities all at once become sinks of filth and nastiness? have the seasons been changed, or have the inhabitants given themselves up at once to swinish intemperance and gluttony, devouring like savages their meat half rotten, half roasted, or half boiled. From some *declamatory publications*, indeed, one might be apt to think that the authors certainly meant to bring such accusations against them. But it undoubtedly will be found an hard matter to prove that the general cleanliness of the country is inferior to what it was, or that the people are

less virtuous than they were before. Besides has not the vigilance of the magistrate ever since 1793 been exerted to the utmost to procure a removal of these nuisances from which the disease might be supposed to arise. No wonder then that people unable to see the causes of these things, should have recourse to something invisible, which they called the constitution of the atmosphere." *Treatise on the Plague and Yellow Fever*, p. 473. This gentleman from stating the case fairly, has been considered by the authors of these "declamatory publications," with an evil eye; but until they render their own opinions consistent with these obvious and very remarkable facts, their opinions must be considered as mere sophisms. I have had much interesting conversation with gentlemen who visited New York in 1805, in which I have been astonished to find that no grounds exist for the hyperbolical representation of the state of that city given by Dr. Miller. Among these is Dr. John Stewart, whose accuracy and impartiality of observation have never been and cannot be questioned. (See his letter in the Appendix, No. 3.)

It is unnecessary to follow Dr. Miller through the labyrinth of reasoning intended to support the proposition that the epidemic of 1805, and all the preceding similar ones, have been of domestic origin, and of course nearly related to the remittent bilious fevers of the country, for until the foregoing objection is removed, he has no right to claim the conviction of the public. Some general observations published by Dr. Gilbert Blane are, however, well deserving of the best attention of Dr. Miller. See *Edin. Med. Journal*, V. iii, p. 388. These observations, joined to those I have stated to you in the preceding sheets, may lead to a more correct view of the nature of “yellow fever,” (malignant pestilential); they may more especially direct his attention to the principle of unsusceptibility, as it relates to the action of this contagion on the human body; and a more careful investigation of the subject, may enable him to discover that the ideosyncrasy of constitution, and the temperature of the atmosphere, are equally necessary conditions in the production of this fever. When Dr. Miller tells us that “none of the truly contagious

diseases derive any additional force from *impure air*," (p. 281), he ought to define the sense in which he receives the expression. I suspect, however, he proceeds on a distinction between infection and contagion, peculiar to the chemical physicians of New York, but which unhappily is not conceivable by "persons of an ordinary share of discriminating power." I have not the presumption to believe myself endowed with more than this mediocrity of discriminating power, but nevertheless, I will venture to found my belief on the general consent of mankind, and to aver that my belief in the present instance is, that this distinction is fallacious and visionary, a vortex ingulphing all the sober dictates of common sense, all the results of unfettered and extensive observation.

Dr. Miller having thus proved himself superior to the universal belief of mankind, since the human mind began to observe, proceeds to state his reasons for rejecting the agency of contagion in the propagation of the fever of New York in 1805. In the

very outset, however, he is unfortunate in his assertion. "No relation is observed between the source of the pretended contagion, and the spreading of the disease to individuals and families." (p. 282.) We have already seen that he directs his remarks in his Report, not only to the fever of 1805, but to "all the preceding similar ones." p. 280. Taking his proposition in this extended latitude, I trust I have shewn, in a manner satisfying to every reasonable mind, that a strict and evident relation has been observed between the source of the real existing contagion, and the spreading of the disease to individuals and families. I am not sufficiently acquainted with direct and pointed facts which support the affirmative of Dr. Miller's proposition, as it relates to the pestilence of 1805 of New York; but there are many circumstances which strongly oppose the negative of it. Thus Dr. Williamson, in his letter to the Editor of the New York Evening Post, of December 31st, 1805, speaks confidently but temperately, "My letter to the Mayor did not express any doubts concerning the utility of the quarantine law, a law

that is somewhat profitable to the Doctor, (Dr. Rogers, the health officer.) *On the contrary, I did think that the weight of evidence respecting the yellow fever was in favour of its being a contagious and imported disease, in which case the quarantines are absolutely necessary.*" This, it is true, is only opinion; but as such it has fully as much weight as Dr. Miller's ipse dixit. But when this is added to the positive proofs of perjury in former instances, and to the evidence of the existence of contagion in the epidemic which prevailed at Philadelphia during precisely the same period, its preponderance must appear to the most prejudiced. The sedulity to be remarked in the narratives of those medical gentlemen of the American States, who maintain the same opinions with, and who address themselves to Dr. Miller, and the other leaders of their party, is truly singular and curious. I have already instanced cases of evasion; let me now present you with one of specious representation of causes, which are well known to be inadequate to the existing effects. Among many which may be selected from the Medical Repository, I shall take

that which first offers itself. A medical gentleman of Alexandria in Virginia of the name of Hall, writes a letter to Dr. Miller, in which he gives an account of the “yellow fever” which prevailed in that place in 1803. He speaks of intensity of heat, which we have seen is one of the conditions necessary to the developement and diffusion of this peculiar infection; and from the imperfect description given, we are made tolerably certain of the presence of the fever which it excites. But when treating of the cause, he dismisses the prevalent and popular opinions by saying, “to avoid prolixity I have declined entering into an examination of the opinions which are entertained by our citizens upon the subject of its origin,” and boldly and inconsiderately substitutes his own, the most inadequate, the well informed physician will perceive, that such an effect could be assigned to---“a kiln burnt in a central part of the city, on which was thrown *a pile of oyster-shells, containing, as was said, some putrid fish.*” How feeble are the supports of fallacious theory! If Dr. Hall had inquired, he would have been informed that in all the towns of

the West India Islands, immense piles of coral, with their inhabitants in every possible state, are burnt for lime, and although the vapour is offensive to the smell, yet in no other way has it been, nor, from the known properties of the gases disengaged, can it ever be, injurious. Where then is Dr. Hall to look for an adequate cause?—not in the soil—“the idea was inadmissible, for Alexandria bid defiance to pestilence; she was destined notwithstanding soon to yield in obedience to circumstances”—and these circumstances were a burning lime-kiln containing, “as was said,” (there was no certainty) “some putrid fish.” Such pathology is beneath criticism, but such is the pathology of Dr. Miller and his party*. See

* The following account which Mr. Cordiner gives of the process for the obtaining the pearls from the oysters, at Ceylon, will be an ample refutation of such absurd pathology :—“When the oysters are in a state sufficiently decayed, and fit to be washed, a portion of them is thrown into a canoe, fifteen feet in length, three feet broad, and three feet deep. The canoe is filled with salt water, in which the oysters are allowed to steep for twelve hours, to soften their putrid substance, and disengage it from the maggots, which float upon the surface, and are easily thrown off.—From twelve to fifteen naked

Med. Rep. Hex. 2, V. ii, p. 18—21. It is a remark which I have no doubt has occurred to you in perusing the Medical Repository, that we seldom see any other publications than such as go to support one side of the question--the side which the editors maintain; if any other is admitted, it is for the purpose of being dismissed with a sneer, with irony, or a direct accusation of misconception, mis-statement, or ignorance. And hence extreme severity and intolerance constitute a remarkable feature in all the critical discussions of the learned and ingenious gentlemen who conduct that work, which have more or less direct allusion to the affirmative of the subject before us;

coolies are ranged along the sides of the canoe, which is a little elevated at one end, so as to allow the water to run off when it is full. The oysters are taken up, one by one, the shells broken from one another, and washed in the water. *The stench proceeding from the canoe during this operation is the most nauseous that can ever be experienced; and a person who is led there merely by curiosity, does not remain long near it. The labourers and overseers, however, from habit become insensible to the smell, and prosecute their business, without expressing any disagreeable sensations.*—Description of Ceylon, &c. by the Rev. James Cordiner, A. M. &c. v. ii, p. 59.

whilst such absurd papers as the foregoing are received with applause. The instance I have just quoted, must bring to your recollection the very extraordinary accounts we first received of the causes of the Gibraltar fever,---such as solar heat rendered intense by reflection from the rocks, and the vapour of burning lime-kilns:---and that it was asserted by the superintending medical officers of the garrison, as Dr. Miller does in his Report, that there never was “any foundation to attempt progressively to trace the propagation of it to any number of persons from the first case, or from any single point of infection.” p. 282. How can these gentlemen reconcile this bold assertion with the facts which have been proved by Dr. Fellows, who instituted his investigation of the cause of this dreadful malady with a mind free from the shackles of preconceived theory?---and how can its prevalence there be accommodated to the very sensible and ingenious speculations of Dr. Mitchell, in his letter to Mr. Varrick, Mayor of New York, on “the excellence of calcareous materials as respects their power to overcome the ex-

citing cause of fevers," (Med. Rep. V. iv, p. 93), wherein he admits that "Gibraltar is famed for its friendship to the health of man, from its being a mountain of marble." How can all this be reconciled, without the admission of a foreign cause, and that cause imported infection?—"Away then with this passion for system building—it is pedantry! Away with this lust of paradox—it is presumption." A great deal of what follows is mere declamation, which may be answered by a very trite indeed, but a very well founded remark—that as the virus of pestilential contagion attaches itself to the clothes as well as the persons of the infected, so can it readily be conveyed to distant and unconnected places, where it will produce its usual effect on healthy persons exposed to it, in the direct proportion of their predisposition, and the peculiar habits of their constitution. There is, however, one observation made by Dr. Miller, which merits more particular attention. "No communication of the disease was ever observed in yellow fever hospitals, *situated at a small distance from the cities to which they belong.* No

exception to this has ever occurred in any of the numerous seasons of this pestilence at our hospital at Bellevue, the marine hospital at Staten Island, that of Philadelphia, &c. *provided the malignant air of the city had been avoided.*" (284). Remark the cautious proviso, the saving clause. Then this malignant air could be diffused "to distant and scattered points," and yet could not extend itself to hospitals at a small distance from its origin. How far the influence of marsh miasms extends, I have inquired in my Essay, V. i, p. 274---283, and shall further inquire in a subsequent part of this letter. Here let it suffice to say, that if such existed in Philadelphia and New York, it would certainly affect the adjoining hospitals. But Dr. Miller extends his remark *to all the seasons of pestilence*, (a word by the bye improperly used if he denies the existence of the very principle of pestilence). Here we have positive facts which prove his incorrectness. There is a very important communication from Dr. Joseph Bayley to Dr. D. Hosack, published in the *Additional Facts, &c.* of the College of Physicians of

Philadelphia, in which these points are clearly established:---the distinction between typhus and “yellow fever,” and “yellow fever” and the bilious yellow remittent---in the symptoms, the causes and treatment. Qu. 1---13. and also p. 32: and secondly, the admission and production of “yellow fever” from contagion in the marine hospital on Staten Island. Qu. 14. See also Dr. D. Hosack’s letter to Dr. W. Currie:---“Many persons from the Irish ships were subsequently attacked with yellow fever, the consequence of being introduced into the hospital where the yellow fever prevailed at the time, or had lately existed. This measure was unavoidable, as they had not sufficient apartments to separate the sick. This fact Dr. Bayley before stated to me that he had seen no black vomit, nor other characteristic symptoms of yellow fever amongst the Irish patients until they had been some days in the hospital, or in other words, had been exposed to the infection. Dr. Ledyard died of the infection of yellow fever received in the course of his duty in the marine hospital.”---The following paper in the same col-

lection is equally important, and still more to the point. From the records of the Pennsylvania hospital signed by Dr. Parke, one of the physicians of the hospital, it appears that five persons caught the "yellow fever" by visiting infected houses in the city in 1798, and that four others afterwards took the disease, from the first mentioned cases having brought the fever into the hospital. Many more received the disease by contagion, but the mischief was prevented from spreading by the judicious measures adopted.—"All suspicious cases were kept apart from the well, and every unnecessary intercourse forbid, which prevented the disease from spreading any further in the hospital." What weight has the bare assertion of Dr. Miller put in the scale against these clear and well authenticated facts. If nothing of this kind happened in the hospitals of New York in 1805, Dr. Miller is reduced to the dilemma—he must admit that he has stated a falsehood, or that the disease was the common bilious remittent; and in either case there will be evidence of a sophism. I may further advert to his argument founded on

the extinction of the disease by cold, which he considers as an “ insuperable objection to the doctrine of its propagation by contagion” (286). Had the disease been typhus, this might be an objection; but it is admitted, that “ yellow fever” is the product of infection, in combination with a high temperature of atmosphere. If it is, and of this there can be no reasonable doubt, cold weather must have extinguished it.

In the former part of this letter, I have called your attention to the unfounded assertion, that persons sick of “ yellow fever” in no instance communicated the disease by contagion, on being removed into the country. In addition to the facts I have already adduced to disprove this, I may refer to the interesting statement of facts tending to prove the contagious nature of the “ yellow fever” at Germantown, in the year 1798, by Dr. Wistar. From one person “ who had been in Philadelphia from the 3d to the 7th August, in a neighbourhood where several cases of the fever had already appeared, who returned home on the 7th, and on the 9th was attacked with the “yel-

low fever," which terminated fatally in four days." Ten cases are specified of the disease excited by contagion emanating from the body of this person. One of these is stated as receiving the infection from sorting the clothes of her deceased daughter; another from the bed on which his mistress died of the fever. The concluding remark of Dr. Wistar is irresistible:—"These melancholy circumstances occurred in a village (six miles and a half from Philadelphia), which has long been remarked for its salubrity, at a time when the other inhabitants enjoyed their usual health. In most of the cases the disease appears to have been contracted at the house of Mrs. Johnson (the mother of the first victim), which before this distressing period, had been eminently distinguished by the health and longevity of its inhabitants. *What cause but contagion is adequate to the production of such a disease among persons so situated?*" This statement is confirmed by Dr. Bensell, and further facts of the same nature are given by Dr. Meredith. See additional facts, &c. p. 36, 42, 47. Now if no facts of this kind occurred, as a consequence, in the country, in the year 1805, at New

York, Dr. Miller must find himself in the same dilemma, as he must do in relation to the hospitals.

In the whole of the paragraph (288) which relates to the exemption of the physicians of New York, there is an incongruity which seems irreconcilable with the general assertion. It would appear that the physicians were and were not exposed to the contagion of the fever, at the same instant of time. What are we to understand from these contradictory passages? “The *universal exemption* of the physicians of New York, amounting to about 50 or 60 persons, from the late disease, is also irreconcilable with the doctrine of its contagiousness. Their *exposure* to the breath, effluvia, and contact of the sick *was almost incessant from morning to night.*” Are we to wonder at this universal exemption, when he informs us, “the more happy escape of physicians in the *late than in former epidemics*, is to be attributed (under the protection of Divine Providence) to their having secured a residence in the higher and safer parts of the town, and to the *comparative*

infrequency of their visits to the districts of envenomed atmosphere." He thus furnishes an enigma which requires the subtle ingenuity of an Œdipus to explain. But on the supposition that this exemption actually happened, it is explained by a general law of this peculiar contagion which exempts those once attacked and recovered, from being again affected by it; in other words, re-infection does but rarely exist in "yellow fever." (See Essay, V. i. p. 233.) Dr. Miller afterwards talks of "the failure of every attempt to arrest the progress of the disease by the separation of the sick from the well," and adds, that the utmost endeavours were used, with the same result, by the Board of Health of Philadelphia. By thus admitting the identity of the New York and Philadelphia fever of 1805, he deprives himself of the advantage of concealment of the truth. In Philadelphia we have seen the causes of this failure, and we have the information in the report of this very Board of Health. "Mistaken, they say, by this opinion (that the disease was not contagious) the people disregarded the regulations and admonitions of

the board, in consequence of which the disease was gradually conveyed into other families, and into streets more remote," &c. See New York Evening Post, Dec. 27, 1805. Every good man must cordially unite with the editor of this paper in the impressive call, "Let the pernicious theorists who reside at New York, consider it well; and let them pause at the awful responsibility they incur, by their blind obstinacy." This most palpable paralogism, "*fundusque mendax*," seems decisive against the tenor of the whole report, for one of two conclusions must result. Dr. Miller must have been ignorant of his subject, or he must have falsely represented the nature of it. In either case he is unworthy of belief—all his sophistry is unavailing; the pernicious theorist stands confessed. This remark (a harsh one I am sorry to say) which I conceive must be the obvious result of the careful perusal and collation of Dr. Miller's paper, necessarily destroys "the evidence of his irresistible facts." If more were required it would be furnished by the fallacy of the observations with which he closes this part of his report. Will any ex-

perienced and candid physician yield assent to these assertions? “Yellow fever cannot be considered as a contagious disease—because, unlike all other contagious diseases, it has no specific character, no definite course or duration, and no appropriate, essential, or pathognomonic symptom; because the supposed contagion rarely operates singly, and in general depends upon the co-operation of exciting causes; and finally, because the miasmata which produce this disease, are more or less noxious as they are more or less concentrated, a property which does not belong to the specific poisons of small-pox, syphilis, &c. If Dr. Miller applies these assertions to the malignant pestilential fever (“yellow fever”) they are invalidated by innumerable facts. Let it be sufficient to say, that infection combined with, and peculiarly modified by a high temperature of atmosphere, constitutes its cause and its *specific character*; that its course and duration are not more indefinite than those of typhus and plague; that its pathognomonic symptoms are well known; (See Essay, V. i, p. 195) that assimilation of the human constitution to the tropical cli-

mate, diminishes its violence, but does not change its course, its duration or its symptoms; that the unassimilated suffers infinitely more, and in it the course and duration of the fever are thereby cut short in most instances, as is not unfrequently observed in the eruptive fever excited by the specific contagion of small-pox. Again, if Dr. Miller aims at the establishment of a line of distinction between this and other contagious diseases, in respect of the co-operation of pre-disposition of the body exposed to their contagion, a limited experience in medicine proves such to be altogether visionary. Who does not know, that the degree of intensity of the action of variolous contagion, on the human body, depends on the temperament and other pre-disposing qualities of the body? Who is ignorant that the same syphilitic poison, which violently affects one, will exert no power over another? Can it be denied that the plague will leave some exempt, but destroy others exposed to its contagion? Innumerable have the instances been, of the same persons resisting the contagion of "yellow fever" at one time, but fatally yielding to it

at another. What can this variety be attributed to, but the pre-disposition being more or less adapted to the peculiar nature of the assailing contagion? or are we to conclude, with Dr. Miller, that “the noxiousness of the miasmata of putrefaction, exhaled by heat, and floating in the atmosphere, explains all these facts, and reconciles all these contradictions” (289). His appeal to his fellow-practitioners, and fellow-citizens, if they are men of veracity and not theorists, like himself, must be fatal to him; and in all quarters of the world, where candour and unbiassed experience are the judges, the appeal must be equally so.

The foregoing remarks are applicable to the second part of Dr. Miller's Report, which is, in truth, an evident *petitio principii*; for when he has proved “the non-contagiousness of the disease,” then, and then only, can “the belief of its introduction from abroad” be destroyed. But waving all other remarks on this singular proposition of Dr. Miller's, let me observe that the nullity of it is proved by the doctor himself. “For more than

fifty years preceding 1795, no importation of the disease into this city was suspected; and it is, indeed, uncertain, whether before that year, the opinion of its importation, at any period of the eighteenth century, had attracted much attention" (290). Are we then to conclude, that until 1795, his "seeds of disease" did not vegetate; that until then, the soil of New York did not incessantly teem with miasmata from its pestilential surface? By what charm or magic was their activity restrained until that fatal period? for we have seen, on the testimony of some of the oldest and most respectable citizens of New York, that at former periods of the eighteenth century, the city was in a much more neglected state than in 1795, and since. How wretched must the present race of its inhabitants be considered, thus selected to suffer under the most direful scourge of heaven! The wondrous sagacity and subtle enquiry of Dr. Miller, may, probably, enable him to answer these simple and obvious questions: and it will well become his humanity and benevolence, seconded by these other penetrating and surpassing endowments, to prepare

his fellow-citizens for, and avert from them future pestilential visitations; when

ruptoque ingens Acheronte vorago
Pestiferas aperit fauces.

as once before happened, when that “specus horrendum” Burlingslip (282 note) was opened, and mirabile dictu, from twenty to thirty peaceable neighbours were knocked down at one blow of its blast of putrid exhalations.

If, during the war of 1756, or that of the American Revolution (291) when thousands of sick were landed from the British fleets, at New York, without the introduction of contagion; and if during a period of peace in North America, unheard of mortality occurred; what is the inference, but that during the former, no pestilence existed in the West Indies; during the latter it did, and was imported into the sea-ports of North America by their numerous trading vessels. Or if we take the statement of Dr. Blane, (see his letter to Baron Jacobi, *Edinburgh Medical Journal*, October, 1807,) and believe with

him, that the prevalent fever in the West Indies, during these periods of war, was infectious, or the malignant pestilential, or “yellow fever,” all these endemists would obtain by the admission, is, that an atmospheric temperature, unfavourable to the evolution of the infection and its propagation by contagion, prevailed at New York on the arrival of the British fleets there. In either case, Dr. Miller’s proposition falls to the ground. This is the inference which common sense must draw from the doctor’s own premises. It, unfortunately for general society, happens, however, that men of business, commercial traders more especially, view objects as they affect their own particular and immediate advantage; and as the fallacious deductions of Dr. Miller please, by not militating against these interests, they gain a ready assent. At former periods the commerce of North America was limited; for some years, owing to a singular concurrence of contingencies, their merchants have become not only their own, but the carriers of the greatest part of Europe and the West Indies. We may well imagine, therefore, the Roman patrician rule of action,

to have equal preponderance among the mercantile interests of the United States,

cave ne portus occupet alter,
Ne Cybiritica, ne Bythina negotia perdas

for

Et genus et formam regina pecunia donat.

Let me here request your attention to the very sensible remarks of our friend Dr. Gordon in the appendix, for they are equally applicable and illustrative.

Dr. Miller (293) says, "in this port, as well as in Philadelphia, a rigid system of quarantine has been in operation for many years, and there is no doubt of its having been vigilantly and faithfully executed."—"The inefficacy of all the various modifications of quarantine hitherto devised in this country, confirm our disbelief of importation." When the premises are proved, then shall the conclusion be admitted. Not only do the premises of this proposition remain to be proved, but there are the strongest possible reasons for being satisfied that their converse is established beyond a doubt; that is, that

quarantine has not been vigilantly and faithfully executed. I have already presented you with the conclusive evidence of an eye-witness both at Philadelphia and New York, infinitely more than sufficient to overthrow the unsupported assertion of Dr. Miller. Until, therefore, this unsurmountable objection is removed, we are not to wonder that “during the last ten years, we have heard ten times more of imported contagion, and of its ravages, at these very ports, than for an hundred years before, when no quarantine was in existence.

The reporter is unfortunate in stating (294) that “the source of mistake on the subject of importation, consists in not distinguishing *a febrile poison generated by heat and filth in a vessel, from contagion taken up in a foreign port, and successively communicated from one person to another.*” One universally admitted fact, I mean in the West Indies, incontestibly proves that *heat and filth* alone do not render shipping “the most dangerous of all human habitations.” The fact is this—that the shipping frequenting the dif-

ferent ports of the West India islands, before 1793, were almost uniformly and remarkably healthy. Had Dr. Miller been acquainted with his subject, or unbiassed by a pernicious theory, he would have known, and honestly said, that it is only when ships lie in such situations as expose them to the malignant influence of marsh miasmata, as at Grenville Bay, in Grenada; or when they are, from existing circumstances, placed in the situation of the Hankey, at Bulama; they become, in the first instance, subject to remittent and intermittent fevers; in the second, to a fever of a malignant pestilential nature, the peculiar features of which will be determined by the atmospheric temperature in which the infection is generated, and which is capable of propagation by contagion. The ships resorting to the port of St. George, of Grenada, were never remarkable for cleanliness, some of the London traders excepted: on the contrary, many of them superabounded in filth. Yet, during ten years' experience, prior to 1793, I could in no instance remark any peculiarly malign consequence. The ship Elizabeth, of Lancaster, furnished, during the

pestilence of that year, a most striking illustration of this. This ship was almost literally a nuisance; every part of her was proverbially filthy, and her crew were equally so, and yet owing to her being the most distant from the focus of infection, the Hankey, she was the last that suffered from it; whilst some London ships, remarkable for their cleanness, owing to their proximity, were the first sufferers. The passage I have here quoted from Dr. Miller's report, is most manifestly founded on theory; and his reasoning on, and his illustration of, the novel proposition it contains, are equally futile and inconclusive. Read and be convinced. "On what ground can a disease be said to be imported, which has no other relation to a foreign country, than that of being generated in a vessel which has lately visited that country? The foreign country, the outward and homeward voyage, are circumstances of no moment in determining the origin and character of the disease; to account for this, we must consider the filth, the moisture and heat, which concurring to a certain degree are destructive to man at all times, in all situations, and under every

condition. And a fever originating under such circumstances, can no more be pronounced imported than a fracture of a limb happening at sea can be called an imported fracture." To understand Dr. Miller, he must construct a Millerian Dictionary, for those in use and received as the standards and expounders of our language, do not define importation—non-importation, nor foreign—domestic.

Dr. Miller (*ibid.*) says, there is an entire want of proof of the introduction from abroad, (I suppose we are to receive the word by his peculiar mode of definition) of the germ of the epidemic of 1805, and this he thinks "gives the last blow to the doctrine of importation." Here again we have to lament this gentleman's limited knowledge of his subject. It often happens, that when the contagion of a pestilential disease has been once imported, (and incontestible proofs of this I have already adduced) its recurrence, at subsequent periods, may not require fresh importation. The seminum of the disease may be preserved in clothes, &c. and when

the temperature of the atmosphere, habits of the inhabitants exposed to it, and other predisposing circumstances concur, then the evolution and action of its virus may, with certainty, take place, and become as fatal as after the original importation. The records of plague, small-pox, and "yellow fever" (malignant pestilential) abound with manifest proofs of this. Is it not, then, more probable that this happened in the United States, than that their sea-ports should be, maugre the utmost exertions of the executive of their government, sinks of pestilence from causes existing in, and inseparably attached to their soil? "Causa latet, vis est notissima fontis." This indeed is remarkably evinced in the fever of 1803, at Philadelphia. Its *germ* could not be traced from the West Indies, and the almost coincidence of its commencement with that at New York, precluded suspicion of its having been introduced from that city. The account given by Dr. Caldwell, a strenuous anti-contagionist, clears up the mystery. It appeared in three points between the 19th and 28th of July, the inhabitants of which had no intercourse. A remarkable circumstance diffused the contagion extensively, and

the more readily by the pre-disposing state of the atmosphere. On this occasion indeed, it would appear from Dr. Caldwell's expression "the attention and sympathy of the citizens were now directed wholly towards New York, where the calamities of *pestilence* appeared to be daily increasing," for "having discarded, in a great measure, all apprehension as to their own fate during the remainder of the season, they felt more acutely for the distresses of their neighbours," that a new infection, derived from intercourse with New York, may have had considerable share in extending and strengthening that already lurking in the city. The circumstance I allude to, was this:—On the 25th of August, nearly a month after the fever had partially appeared in the points of preserved infection, a dreadful fire broke out near Market-street, which drew together a vast concourse of people. "Such a scene was well calculated to act on the exciting cause of disease" where infection already existed. Accordingly, three days after, "the disease appeared suddenly and nearly at once in five or six families, the individuals of which had probably never exchanged

a visit or even a word either on business or ceremony.” In a week after this fatal course, the disease gained strength and extension of attack. On the 12th of September, it raged with such violence that the Board of Health interdicted all intercourse. In October it reached the more elevated parts of the city; and finally, as usual, a change of temperature either extinguished or suspended it. Dr. Caldwell, however, like our present reporter, being inclined to weaken the force of the foregoing facts, contradicts his own statements;—p. 148, he spreads the disease to a distance of more than a mile, and p. 153, he says, “it was no less circumscribed in point of time than of situation.” It is thus these gentlemen vainly endeavour to blind their fellow-citizens, by specious representations; that thus they betray the weakness of their cause. See *Med. Rep. Hex. 2, V. i. p. 143—155*. Some very acute and sensible observations of M. Sabatier on the means of promoting the health of New York, throw much light on the latent evil, particularly his remarks on the lodging houses of that city, where, he says, “probably is deposited the

seat of the disorder." They are well deserving the serious attention of the magistrates. The nuisances of docks, &c. which exist in even a greater degree within the tropics, without experiencing from them any of the imputed pestilential consequences, is of little or no comparative importance. See Med. Rep. V. vi. p. 37. I am confident the purposes of prevention might be more completely fulfilled by paying that attention to low houses of dissipation and lodging, the resorts of misery, indigence, and vice at all times; and during pestilential seasons, the secure asylum of infection, which, with fruitless assiduity, is now lavished, on the slips, sewers, docks, and made-grounds of New York. The assignment of the calamity to *made-ground* appears, indeed, particularly singular, most elaborately strained. The making of ground is often, I believe generally, the filling of marshes, or the elongation of solid surface for facilitating the objects of commerce, by which the exhalation of putrid miasms from the former, being prevented, and the principles from which they arise, being decomposed and

absorbed, a surface remains once useless or injurious, now habitable and productive.

In a report of the nature of Dr. Miller's, which should be calculated for general information, and general precaution, every circumstance capable of promoting or restraining the diffusion of disease, should be carefully noted, and ingenuously promulgated: it should have the general good for its object; theory should yield to practical knowledge; and the interests of a party should have no admission. That this has not been done, in the present instance, is most obvious. A most important consideration in the developement of the causes of pestilence, is the radius of its contagion. It is pretty well ascertained, and, I believe, pretty generally known, among physicians at least, that the radius of pestilential contagion, varies, according to its peculiar nature, from almost contact to five or six feet; that of "yellow fever" generally six feet. See Essay, V. i. p. 309. Now as the contagion of the fever of 1805, at New York could be propagated only by the approach of the healthy to the sick, within the

contagious limit, and as the knowledge of this important fact must, we should imagine, have been impressed on the minds of the inhabitants by former, recent, and reiterated experience, how can we perceive astonishment that the disease should be confined to nearly the spot in which it originated? The appearance of the disease in another opposite quarter, we shall presently see, is accounted for by Dr. Miller himself, on the well known and received principle of fomites. Another consideration of equal importance has been either entirely overlooked, or sophistically perverted to the support of the theory, which seems to have been the main object of the Report. It is highly probable, indeed almost reduced to a certainty, that the radius of contagion may be lengthened or shortened by the medium through which the contagious miasms pass. Thus an atmosphere deprived of its oxygen in a very great degree by the respiration of many persons assembled together, lessens the tendency of the basis of these miasms to decomposition or solution: thus too the consumption of oxygen, by combustion, produces the same effect, as we are

told was fatally experienced in the plague of London, of 1665. This may happen in an unconfined space as readily as in a small chamber, when the number of persons collected together, is proportionally great, an instance of which we have seen occurred in the pestilential season of 1803, at Philadelphia. This will be more perceptible if the surface of the former is closely built on, and the houses disposed into streets, or ornamented with large trees or extensive shrubberies, for in these cases, the uninterrupted lines of houses, or the trees, have a corresponding effect to the parietes of the latter. On this principle alone, can the communication of infection to a considerable distance in crowded courts of justice, be accounted for; and it explains the facility with which it spreads in families possessing very limited accommodation, although the distance of the healthy from the focus of infection, may greatly exceed its natural radius. On the other hand it satisfactorily displays the cause of exemption of the healthy, in the vicinity of others labouring under the epidemic of 1805, in New York, where the streets and houses are

spacious and well ventilated:—and in general for its very limited diffusion during that season. Another cause of exemption in cases of constant exposition to the influence of pestilential miasms, is habit. If from ideosyncrasy, or other fortunate circumstance, the first shock is resisted, habit renders all subsequent shocks less violent, until at length they are deprived of all power over the system: the nurses and menial attendants are more especially acted on by this law. These very persons, thus happily circumstanced on one occasion, may however, on another, become the victims of infection. Many examples of this occurred during the years 1793, 4, and 5, at Grenada; and had they been marked and recorded with equal candour and fidelity, I have no doubt many would have been observed, during the epidemics of Philadelphia and New York, since 1793.

Let us take another view of the subject: let us admit, for a moment, that marsh miasmata, or “the miasmata of putrefaction exhaled by heat, and floating in the atmosphere,” were the causes of the epidemic of 1805, at

New York, and let us see how far this supposition is reconcilable with the facts stated by Dr. Miller. He speaks of sporadic cases of the disease (282) being found scattered at distant and unconnected points; and starting up singly in situations where contagion could neither be traced nor suspected; and this proposition he illustrates by an incident very irrelevant. “The blast of putrid exhalations from the sewer of Burling slip,” cannot be considered as conclusive: all that results from it is mere vague conjecture (supposing its effects to have actually happened), seeing the epidemic of 1798 had already prevailed. In 277 he gives a very different statement, viz. “during the early period of the epidemic *nearly all the cases* took place on the eastern side of the city, in Front, Water, and Pearl-streets, and principally below Burling slip. They afterwards became more generally diffused.” We find (*ibid.*) that the putrid exhalations of the north river side did not issue till about the 20th of September; “the miasmata came to maturity on the one side two or three weeks sooner than on the other,” (*ib. note*) and on one occasion, 1803, they are

made to proceed from infected shipping; on another, 1805, from “made-ground.” On both sides, it seems, there is made ground, but the inhabitants of the north river side have been more circumspect, by making choice of “materials less foul and corrupt.” How vague and inconclusive! how little consonant with the experience of reasonable and candid men, who have resided where such miasms have actually existed! Again, as a proof of the non-contagiousness of the epidemic of 1805, he offers this fact—“in the mass of six hundred cases reported to the Board of Health, there were only thirty-five houses in which more than a single case was found.” If this be a fact, for we have only the doctor’s assertion, the consideration of the general laws of contagion, as well as of their applicability in the present instance, will, I presume, rather tend to prove it an evidence of the prevailing infection, than of marsh miasmata. For on the supposition of the inhabitants being stationary during a given time, and we must necessarily suppose that this happened, before the alarm became general, in the vicinity of “*a vast hot-bed of putre-*

faction incessantly teeming with miasmata," the calculation of result, founded on the data of experience in all similar circumstances, will be that these miasmata will be more productive of disease than contagion among them, in as much as the influence of the former is extensive, of the latter very limited. It is absolutely incredible, seeing it is totally inconsistent with our knowledge of the laws of marsh exhalation, that only one of a family should suffer by so profuse an expansion of morbid cause; an expansion which set at nought "currents of air," and "loading with the seeds of disease *every successive portion of atmosphere.*"

Dum flammâ sine thura liquescere limine sacro,
Persuadere cupit; credat Judæus Apella,
Non ego.

I am aware of the conclusion you have established in typhus and small-pox, from an accurate induction from facts, viz. that 22 in 23, or 32 in 33 are susceptible of infection, providing they "*are sufficiently exposed to the poison.*" See Haygarth's letter to Dr. Perceval, p. 31, 35. This conclusion, I be-

lieve, is indisputable; but instead of contravening, it supports and establishes my proposition. Further, Dr. Miller's statement (287) disproves his general assertion respecting the local origin of the fever of 1805. He says, "*In the beginning of September* a considerable number of sick, who had taken the disease on the eastern side of the city, were removed to the western side, where they died with *the most pestilential symptoms!*" How is this to be reconciled, without the intervening agency of contagion, with the preceding information (277). "About the 20th of September, they began to prevail near the north river;" that is, the west side of the city. The period here given from the removal of the sick to the appearance of disease in new subjects, exposed to infection, on the western side, corresponds with sufficient exactness, to that, during which contagious poison remains latent in the system.*

* One of the first measures I adopted in this inquiry, was to frame a set of queries and remarks which resulted from my perusal of Dr. Miller's report, and to transmit them to Dr. David Hosack, senior, of New York; with a request that he would take them into his consideration, and favour me with

Towards the close of the Report (296) its real design becomes prominently conspicuous.

the answers and observations, which his long residence in New York, and very extensive field of experience during the periods of the prevalence of the pestilence, enabled him to give. Had these answers been received, they would, no doubt, have constituted a most valuable and satisfactory refutation of Dr. Miller's statements. My letter, dated in July, 1807, by some unaccountable means, did not reach Dr. Hosack for many months; and it was not till August last, I had the pleasure of hearing from him. He reserves himself for a letter which he proposes to print as an answer to my queries, in which, he says, he will expose, by documents of the Health Office, the want of candour on the part of Dr. Miller in his letter to the governor. The letter I have received, although, I hope, only the precursor of a more important one, contains valuable evidence of the justness and correctness of my strictures, and the public have therefore a right to the communication of it.

Dear Sir,

New York, July 9th, 1808.

Your valuable communication to me on the subject of yellow fever, I received two months ago. I should have replied to it before, but my engagements in the college, added to an extensive practice, so fully occupied my time that I have not had leisure to write scarcely a page on that or any other subject. I have concluded also, for another reason, to defer my answer until the autumn, when I shall have the advantage of some additional observations, the result of the present season:—*not that we have yellow fever prevailing, but that we have thus far experienced one of the hottest seasons ever known, and proverbially one of the healthiest.* This year's ex-

The advantages of commerce to a rising trading nation, are not to be sacrificed at the

perience will add many converts to our doctrine of the specific nature of yellow fever, and present an unanswerable difficulty to the advocates for the domestic origin of this disease, and its identity with the bilious remitting fever. My answer to your queries I shall print in a letter to you, and in that shall expose, by the documents of our Health Office, the want of candour on the part of Dr. Miller in his letter to the governor, and to which your statement refers. I allude more especially to that part of his paper where he enumerates the different streets and numerous parts of our city in which the yellow fever made its appearance, and which he is careful to recite as an evidence of the domestic origin of the disease. As a member of the Board of Health he must have known that the disease was confined for many weeks to a small portion of the eastern side of the city, and that not a case occurred in any other part of the town that was not referable to that as its source. Such was the statement of the Board of Health to our citizens, and in consequence of which they forbade intercourse with the infected portion of our city, and ordered an abandonment of that part of the town, threatening violent measures if their orders were not immediately complied with. A few weeks after the infection extended a few streets further. The Board of Health accurately defined its limits, and again declared that still not a case occurred but could be traced to this spot of the city as its source. Dr. Miller carefully enumerates the cases occurring and the numerous parts of the city in which the sick reside, but as carefully suppresses the observation of the Board, of which he too was a member, and must have known, that the persons so taken sick, had, prior to

shrine of humanity, to the prevention of fatal maladies, the preservation of health, life, and

their attack been exposed to the infection, by frequenting the infected spot. In my printed answer I shall incorporate much of your communication—or shall I print it entire? Let me know your wishes; your communication should be published in some form or other. I wish you could visit the United States; you would find materials for giving a final blow to your enemies*:—three or four months' residence here would be sufficient.

Accept, dear Sir, of my esteem and respect,

Yours,

DAVID HOSACK.

Dr. Chisholm,

Clifton.

I shall here take occasion to remark that Mr. Tytler (whose acute observations on the subject have been quoted in the preceding sheets) we find is not singular in making them; indeed they must occur to every thinking man. Mr. Royston in his excellent and well digested "Sketch of the progress of Medicine, for the year 1807," published in the London Medical Journal of July, 1808, thus fairly draws the result of a comparison of Dr. Blane's letter to the Baron Jacobi, and Dr. Miller's report to the governor of New York. "The extensive views which Drs. Miller and Blane have taken of

* I feel deeply concerned that this word should be thus applied, for from my heart I disclaim all enmity, except against false doctrinal opinions, in religion, morality, and medicine, the three most interesting subjects to mankind, on which their future and present welfare depends.

all that renders life desirable. “Under the influence of this phantom of contagion,” says

this epidemic fever, leave still a doubt of its immediate cause. It is inconceivable that the permanent source alledged by Dr. Miller, and arising out of fixed localities, as at New York, should so seldom produce the disease. How has it happened that the extensive pabulum of ninety acres of ground lying along the east river, “*originally composed of the most corrupt materials, and pregnant with almost annual pestilence,*” so rarely produces yellow fever. If this proceeds from a long series of years being deficient in the essential degree of heat, let it be shewn by actual registers of the weather during such periods. (Let this be compared with Dr. Hosack’s prophetic observation above). “Has it arisen from long calms, combined with the high degree of temperature? let it be ascertained that the natural conjunctions of calm and heat had not occurred. For thirty years Philadelphia was free from this visitation. Can it be ascertained that the natural conjunctions of heat and calm were in the year 1792 (1793 it should be. Mr. R. was led into the error by Dr. Blane’s mistake, which that gentleman, in his letter to me of January 2, 1808, has thanked me for correcting) precisely what they had been in 1762, and that in the interval of thirty years no coincidence of atmospheric influence had arisen to call this pernicious vapour from its putrid resting place? It appeared at Cadiz twice within an interval of thirty-six years. Can it be ascertained that in 1764 and 1800, the heats and calms in the atmosphere at Cadiz were similar: that the exhalations from the earth in those years had the same qualities: that the inhabitants were accumulated in a like degree: that the want of cleanliness in the town was to the same extent: and that in the intervening

he, “ we have instructed the Europeans to enact laws and regulations, sanctioned by the highest penalties, which retard and oppress our commerce, and subject our shipping in their ports to the most grievous detention. To crown the whole of this injury and humiliation, we have instigated them to place the people of the United States, by late extensions of quarantine, on the same footing with the *degraded and detestable* inhabitants of Barbary, Egypt, Syria, the Archipelago, Con-

thirty-six years; no such concurrence of incidents happened? Why has the disease occurred but once at Gibraltar? It is in part answered by the autumn of 1804 being hotter than any other in remembrance. But did the thermometer never stand at eighty and upwards in any other autumn than that of 1804; or were the secondary causes of fear, fatigue, cold, intemperance, &c. more cogent in that year, than in any preceding period? Do the localities of Gibraltar agree with Dr. Miller’s first proposition, that “ *the pernicious exhalation arises from low and moist grounds?*” It seems a natural inquiry why this pernicious vapour “ *the most universal cause of disease in nature,*” so seldom operates in those countries where typhus icteroides occurs in a most unequivocal shape; and why, from so universal a cause, evident effects are not produced in places affording every collateral circumstance for their generation? These difficulties are all obviated by admitting an imported contagion at a period when the secondary causes are in full power,” &c. p. 33.

stantinople, and other parts of the Turkish dominions; and all this has been done in defiance of *clear and luminous facts*, and in the face of *long reiterated and ample experience*" (297). Cicero, you know, recommends the avoiding of two prominent faults, to the cultivator of science:—not boldly to assent to and prefer things unknown, to those which are known and admitted—and not to bestow much attention and labour on things naturally obscure and difficult, more especially when the knowledge of them is not necessary. (Ciceronis offic.) Into these faults I am much inclined to think Dr. Miller has fallen in the instance before us. We perceive it in his own answer to the following question. By whom have the people of the United States been placed on the same footing with *the degraded and detestable people* of Barbary and the Turkish dominions? He tells us by "the acts of their own state legislatures, the proceedings of their municipal bodies, *and boards of health*, the proclamations of their magistrates, and by a variety of other public documents" (296). It is certainly more than presumable that what they saw,

and used their utmost endeavours to guard against, must have been palpable and obvious, not difficult and obscure; that the knowledge of these well ascertained evils, was important and necessary, and that it was totally unnecessary to look to those local circumstances conjured up by speculatists, for their origin. These were, therefore, very properly neglected; those had all the attention due to their importance, bestowed on them. The inference is therefore this; the legislators, the executive, the health officers of the United States are fools and visionaries, and Dr. Miller and his cabal (shall I say?) are supereminently endowed with perceptive and penetrating talents—very “*convivæ deorum*,” and Miller himself “*Jovis arcanis Minos admissus*.” I leave you and my readers to reflect on this. To return to the subject more immediately before us. How far “the discarding the bugbear of contagion” will ascertain the nature of the prevailing epidemic, secure personal safety, and improve the measures necessary for the salubrity of the city of New York, must appear to every thinking, impartial, and well-informed physician, at least,

extremely problematical. The truth is, I apprehend, general and individual safety has been endangered by the imperfect knowledge of the laws of contagion, on the one hand, and the unjustifiable criminal prejudices and speculations of the leaders of a party, on the other. For if the causes of the epidemic are local, as Dr. Miller labours to make us believe, the danger must necessarily be augmented by the vicinity of the inhabitants to "a hot-bed of putrefaction;" if they arise from contagion, there can be no necessity for "that desertion and misery of the sick which have too often disgraced society in every epidemic" (297). But declamation will do no good, and such as the following, on the mass of mankind, may be productive of infinite evil. Common sense, sound discriminative judgment, and the candid pen of history, are equally abused and insulted by the promulgation of such absurdity. "We live in the latitude of pestilence, and our climate now perhaps is only beginning to display its tendency to produce this terrible scourge" (298). Fortunately, however, the poison in this instance, carries with it its antidote to men of

discernment, for it is a direct contradiction to the author's former assertions, which I have more than once pointed out.

From the minute consideration of this most important subject, and the enlarged view of all its circumstances, we have taken in the preceding sheets, there arises, if not a certainty, at least the strongest probability, that the original appearance of the malignant pestilential or "yellow" fever, proceeded from imported infection; that its subsequent diffusion arose from contagion, and that its frequent recurrence at distinct periods, may be accounted for in a manner perfectly satisfying to the reasonable enquirer, either by the importation of fresh infection in some instances, or, in others, by the evolution of the basis of that which had already devastated the cities and towns of the North American States, but whose operation had been suspended by restraining, not extinguishing causes. If this is the result of our enquiry, it follows, that whilst rational quarantine laws are fully and rigidly acted on to prevent the admission of fresh infection; to obviate

that already existing, the establishment of institutions for the reception of patients under fever which may be considered as at all suspicious, should be resorted to, and enforced with all the energy and wisdom of the government, and all the prudence, the humanity, and stimulus of personal safety, of the more opulent inhabitants of the United States. The encouragement held out by the salutary consequences of such institutions in other countries, should not be neglected, should not be contemned, nor give place to notions of security truly ideal, and which have so often been subverted by fatal experience. Let the wise measures of prevention be adopted which have been so successfully pursued in most of the cities and large towns of Great Britain and Ireland, particularly Chester, Liverpool, Manchester, and Dublin, and then, indeed, contagion may be rendered “a bug-bear;” then indeed “we shall lessen apprehension and distress, we shall disarm the evil of half its power, and restore the ties of kindred and of nature.” I most cordially join Dr. Miller, therefore, to the fullest extent of his language, when he thus addresses

his countrymen. "It is surely time to investigate this subject with the deepest attention, and to adopt *some adequate system* of relief. The warning voice of history and experience loudly calls us to make every exertion to *deliver our city from nuisances*, which threaten to entail the miseries of an annual succession of malignant epidemics" (297). When it is further considered that "houses of recovery," as they have been called, have been established with a success more than commensurate with the most sanguine hopes of the projectors, in the cities and towns of this country, most renowned for commerce and manufactures, a still more cogent incentive will be held out to the legislators and the principal inhabitants of the cities and towns of the United States. Feelings of general philanthropy, as well as of particular respect for, and the disinterested hope of promoting the welfare of these states, so cruelly suffering under the ill-judged promulgation, and, I fear, adoption, of a most pernicious theory, excite in me the wish that they may cordially relinquish all the animosity and spirit of party, and unite in the ac-

complishment of this great, this beneficent object; which I feel a reasonable confidence, will be productive of a permanent freedom from this most dreadful of evils.

Some papers illustrative of the statements contained in the foregoing letter I subjoin. They are partly furnished by yourself, and are partly the product of the accurate observation of the late Dr. John Gordon, of St. Croix. Having lengthened this letter much beyond my original intention, I hasten to assure you of the respect and esteem,

My dear Sir,

of your very faithful friend,

and humble servant,

C. CHISHOLM.

APPENDIX, No. 1.

*To JOHN HAYGARTH, M. D. F. R. S.
London and Edinburgh, &c. &c. Bath.*

SIR,

THE following relation of the appearance of the contagious disease commonly, but very improperly, called the yellow fever, by some the Boulam, and which I shall distinguish by the name of the pestilential fever, is given only to the best of my recollection, having no papers or documents with me to refer to. But the facts are recent, have made too great an impression on my mind ever to be forgotten, and could be testified by numbers of living witnesses.

Besides they could be proved by the me-

morials I sent at different times from the year 1793 to 1803, inclusive, to the Danish West India government in St. Croix, which by them were transmitted to their corresponding departments in Copenhagen and still are preserved in their archives.

The pestilential fever first appeared in St. Croix in the month of September, 1793. It was brought there by the brig Lougen, belonging to the royal Danish navy; to the crew of which the infection was communicated on the following occasion. A hurricane, or rather a violent gale of wind from the south drove on shore most of the vessels that were lying in the harbour of St. Thomas. The brig was sent by the government in St. Croix to assist the sufferers, and the sailors from being obliged to go on board the different vessels had communication with their crews. Some of the vessels were from Grenada and other English islands, where the pestilential fever was then raging, and had done so since the month of March preceding. The brig was perfectly healthy at her departure from St. Croix, and had been so since

her arrival in the West Indies, not having for several months lost a man.

The weather was rainy, stormy, and uncommonly warm. Fahrenheit's thermometer seldom being as low as 83, and sometimes rising to 93. After a fortnight's absence the brig returned, and landed immediately thirty-two sick. She had lost six in St. Thomas and I believe left some too dangerously ill to be removed. The sick were all under my care, and in the course of three weeks amounted to sixty-two of the inferior part of the crew, of which number seventeen died, and two cadets of the navy out of five or six officers infected. Of the whole complement, which, including officers, amounted to eighty-four men, only four escaped infection.

From the brig it was communicated rapidly to most of the European and North American vessels in the harbour, the crews of which were generally healthy before her return, and were more or less infected in proportion as they were more or less accustomed to the West India climate. On that account the

crews of the droghers or coasting vessels were but partially infected.

The soldiers in the garrison were in the same predicament. New comers were infected with the pestilential fever, while the old seasoned soldiers had only the common tropical remittents, and this was universally the case whenever both diseases were at the same time epidemic.

The lowest proportion of deaths among the infected of the shipping was one-third of the number, the same as of the brig above mentioned. Some vessels, however, lost one half, others two-thirds, and one four-fifths of the number infected, owing to the want of timely application and care. The disease raged until the middle of November, and then ceased suddenly and intirely, whether owing to the shipping having left the port, to the means used for eradicating the infection, or to the coming on of stormy and cold weather from the northward. To the latter cause I attributed principally the sudden cessation of the influence of contagion.

The thermometer before sun rise, and at nine p. m. was sometimes at 73, and rose about noon to 83; it was, however, generally below eighty, as long as the north wind blew.

There was no further appearance of the disease until the month of July following, when a merchant ship, the *Sovereign*, Captain Story, from London, came into the harbour of Christianstadt. She had delivered goods at two islands in her passage; had in one of them received the infection, and lost in St. Croix three out of five in whom the disorder appeared.

They were attended by another medical practitioner, but two ships that were lying, one on each side of her, were under my care, and in them I could evidently trace the origin and progress of the infection from the crews having intercourse with one another.

From these ships the infection spread as in the former year, but the number of the infected, and of deaths was less, as early ap-

plication and other precautions were more generally attended to. I left the West Indies in September, 1794, and did not return until June, 1796. In my absence the pestilential fever had committed great ravages among the shipping.

It appeared at various times in the hot months of 1796, 97, 98, and 99, and was principally introduced by American vessels, and those from St. Thomas, but more frequently by the former. We were on our guard against it, and whenever the fever appeared we traced it to its source, turned the vessel out of the harbour that brought it, cut off all communication of the infected with others, and if they had been brought on shore, or the disease any where appeared, took every precaution to prevent the further spreading of its malignant influence. Hence, though frequently imported, it was never general in the years above mentioned.

In the year 1800, a pestilential fever of exactly the same nature, but rather with increased malignity again appeared, and was

communicated very generally to all who were susceptible of its contagion and came within the reach of its influence.

In January, 1800, part of a squadron of cavalry consisting of forty-two privates and three officers arrived at St. Croix, and with them eighty recruits for the infantry of the different garrisons, of which thirty remained at Christianstadt, the capital of the islands. The rainy season had been uncommonly dry, and the weather warm.

There had been very little sickness and only one death of a phthisical patient in the cavalry in the whole garrison for six months, consisting in the latter end of March of about 160 men. At this time a mortal disorder attacked the new comers, and was at first attributed by me to a low marshy situation in which a house rented for the cavalry, as a barrack, contrary to my opinion, was situated. The fever, I thought, was the usual remittent occasioned by marsh effluvia, but its rapid progress and great mortality soon undeceived me;—too late, however, to save the lives of

many, for the first eight that were attacked all died. There were several sick sailors in the hospital, and to their comrades who visited them the fever was communicated.

From them it spread very generally among the shipping and inhabitants of the town, who were unaccustomed to the climate, for the Creoles, or those who were, as it is called, seasoned to the West India climate, were not as far as my experience goes susceptible of the infection of that pestilence. I never saw a negro affected by it, and but one Creole whose case was dubious. Dr. Chisholm has given, as an assertion of mine, that the disease in the year 1793, was not communicated to the inhabitants of the town, though several of the infected were brought to the hospital, and some to private houses. It is to be understood with the above restriction, for several new comers from Europe and North America suffered in that epidemic. Likewise in those of the year following, and March, 1800. In those of 1796, September, 1800, and August, 1802, it was confined to the

shipping, and in the last the garrison was infected.

Of the sailors not one in four died. It was otherwise with the cavalry and infantry recruits. Of the former in the course of a month out of forty-one privates, nineteen died, and two of the three officers. Of the latter thirteen out of thirty. The cause of this mortality was, in the first instance, my not being aware of the infection being introduced, as I had no grounds for suspicion, and more particularly the aversion of the cavalry and recruits to medical treatment.

Both officers, and most of the men were in the most dangerous state before my assistance was permitted or required. The source of the infection was at first difficult to be discovered, but I was fortunate enough to trace it; two men were sent in exchange from St. Thomas to our garrison. They were recruits, had gone through the fever in the hospital of St. Thomas, and came directly to us from thence in a convalescent state with the clothes on them which they had worn during the

time of their sickness, for no hospital dress was allowed.

These men were so weak on landing that they were immediately sent to the hospital without my knowledge, and from them certainly originated the pestilential fever. The proofs upon which this conclusion rests are: that it first appeared among the sick in the hospital, who were not in any dangerous state on their arrival, but were all convalescents from the remittent fever and other diseases. Besides, at that period there was a great mortality in the garrison at St. Thomas, while in ours not a man had died for six months.

This circumstance struck General Lindeman, the governor and commander in chief, and he inquired of me the reason. I answered that it could be accounted for in no other manner but from the operation of contagion, for the state of the atmosphere was then, and had been for some time very favourable to the general health of the islands.

There had been long a difference in opi-

nion respecting the nature of the pestilential fever among the practitioners, both in St. Croix and St. Thomas. In the latter, particularly the garrison surgeon and others persuaded the commandant that it was not contagious, and consequently, that otherwise respectable and enlightened officer did not think proper to allow of measures being taken to prevent its importation or communication.

For several years after my most urgent remonstrances had prevailed on the government in St. Croix to establish a species of examination and quarantine for suspected vessels, no such institution was adopted in St. Thomas, so that vessels driven from our port on account of evident infection on board, went directly over to St. Thomas, and were allowed free entry.

This difference in opinion, and consequently of proper measures, was the occasion of much disappointment to me in my endeavours to prevent the importation of infection, for the government in St. Croix, equally jealous as that in St. Thomas, of any obstacle to trade,

or to the prejudice of the revenue, would allow of no interruption to the intercourse between the two islands, and vessels of all descriptions passed daily from one to the other, without examination. This jealousy was heightened by the artful policy of the interested, who magnified the pretended obstacles to commerce, while in reality though they had been sufficient to prevent the importation of infection, would have occasioned very little general interruption to trade, and only a partial sacrifice when absolutely necessary, and a small expence in individual cases; proofs of which were given by the measures afterwards adopted, which produced none of the effects pretended. It was owing to the want of a quarantine establishment at Frederickstadt, the second port of entry in St. Croix, that a second contagion was introduced in October, 1800, a few months after we had, by proper means, eradicated the malignant one above mentioned. A vessel belonging to the island and trading to St. Domingo, touched at Frederickstadt, and landed a passenger there sick. She came directly to Christianstadt, and reported that

she only came from thence without mentioning St. Domingo.

A few days after her arrival I was called on board, and found either one or two Americans dangerously ill of the yellow fever, and who died. Though she was ordered immediately under quarantine, yet the pestilential fever soon appeared in the harbour which I have every reason to conclude was imported by the above vessel. Among the infected was one of the king's armed sloop commanded by Captain Fusling. Her crew consisted of sixty men, of whom thirty-nine were infected, and five, or perhaps six, died.

A small proportion to former contagions, and which might be accounted for either from the infection being less virulent, or from the means used being more early applied. (It is but fair to allow that both had an influence.)

St. Thomas was, during the last war, and is at present, the principal point of intercourse between all nations, and consequently the

greatest mart of trade in the West Indies. The great mortality there, and in St. Croix, alarmed the different departments of government in Copenhagen. They sent the reports from both places to the quarantine commission, a similar institution to the Board of Health lately established in Britain for examination and consultation. This commission, composed of several of the medical faculty, captains of the navy, and other scientific men, in their answer to the chancery, expressed the highest approbation of my measures, and recommended that not only they should be strictly observed, but likewise that whatever I might further suggest should be put in practice.

At the same time they did not conceal their dissatisfaction at the accounts sent from St Thomas. Their opinion, and the orders from the departments in Copenhagen, in consequence of it, made the government in St. Croix more determined in their operations for preventing the importation of infection, but it was not until the year 1802, when the islands were given up to the Danes by the

English that quarantine establishments were made at St. Thomas and Frederickstadt, eight years after they had been adopted by my recommendation at Christianstadt, the capital. General Walterstorff, the commander in chief, upon being applied to by me for that purpose, answered, with a liberality that does him credit and would equally do so to all those who are biassed by an opinion so fatal to mankind, “ that though he was not convinced that the disease dreaded was infectious, but was inclined to opposite sentiments, yet as he was liable to errors he would take the safest side, and strictly follow the directions given by the quarantine commission in Copenhagen, and enforce the measures they recommended.” This he accordingly did, and I had the satisfaction afterwards to learn that this distinguished officer, by the accounts he received from the different places in the West Indies and America, was convinced of the existence of the contagion of the disease as well as its fatal effects.

But, notwithstanding strict measures were taken to prevent the importation of the infec-

tion yet it again took place, and that in consequence of a deception on the part of the master of a vessel, perhaps owing to ignorance. In one of the warm months of the year 1802, the exact one I do not recollect, a brig from Guernsey came into the harbour, and was reported to have come directly from thence, though she had touched at several of the West India islands in her way.

Guernsey not being reckoned a suspected place, as the pestilential fever in Europe had never extended to the northward of Spain, it was allowed to enter without examination, on declaring that it had lost no men on the voyage, and the crew were healthy. Shortly after its arrival several of her crew were taken ill; it landed four of them in a merchant's court yard, who all died. Being then in the country for the recovery of my health, a physician, who acted as my deputy, saw two of the sick, and declared the disease to be the pestilential fever. The vessel was immediately ordered out of the harbour; all communication with the harbour or shore, except by a single boat appointed for the purpose of

supplying it with necessaries, entirely cut off, and the clothes of the deceased immersed in water, and their bedding thrown into the sea and sunk or destroyed by burning.

Shortly after the pestilential fever appeared in the garrison and in the harbour, and raged in St. Thomas. We could not, as in most of the other contagions, trace it step by step to the above mentioned Guernsey vessel, but have reason to conclude that it was imported by her, and gave us another fatal warning to be upon our guard against false entries and reports.

We had been deceived before, and at other times, upon being on our guard, have been forced to extort the truth by threats of the severest penalties.

It is not meant as a national reflection to remark, that the Americans gave us most reason to be circumspect, as it would apply to the people of every nation under similar circumstances.

Their trade is not confined to one or two islands, as that of the Europeans generally is. They touch often at almost every island in seeking for a market, though they do not enter into any port, yet they have communication with the inhabitants for the purpose of getting information, purchasing necessities, or disposing of their little adventures. Nothing is more easy to conceive than that the sailors might receive the infection without being aware of it; and it has certainly to my knowledge happened so.

They have come from America perfectly healthy, touched at an infected island, there received it, and brought it down to St. Croix, without knowing their danger. Some men there are, who concern themselves very little what effects their conduct may have on their fellow citizens, provided they can escape detection, and accomplish their views of gain and interest. And such are mostly to be found among those who carry on a forced or illicit trade. With regard to such people it particularly behoves the police of every country to be upon their guard against the im-

portation of infection. The progress of the above pestilential fever was soon arrested by the means usually practised, such as separation of the infected, and prevention of their intercourse with the healthy, or the communication of the infection by means of their attendants, who were all persons not susceptible of receiving the infection, whose clothes were continually exposed to the air and kept clean. This, with the purification by air and water of the sick themselves, their apartments, clothes, bedding, and every thing that could be subjected to agency of these elements, was sufficient, in most cases, to eradicate the infection, or prevent its spreading beyond certain limits. The strictest cleanliness was observed, particularly respecting the discharges and excretions from the patient. Their linen, bed-clothes, &c. bedaubed with them were immersed first in cold water, then washed clean with warm soap and water, or alkaline solutions. These were preferred to salt water or vinegar, on account of their uniting with the animal filth and deterging and removing it better. Fumigation with vinegar and some aromatic substances were employed, in com-

pliance with custom, likewise with gun powder and tar.

When the sick were numerous, and to purify their apartments after the disease a liberal use was made of the nitric and muriatic acids, as directed by Dr. Smith and Mons. Morveau.

What effects these latter had in eradicating infection, superior to strict cleanliness and ventilation, I do not pretend to determine, but am not inclined to allow them that credit which their inventors have assigned to them.

White washing the apartments frequently was a necessary part of cleanliness, and practised, in my opinion, with utility.

Though I left St. Croix for America, soon after the landing of the British troops in 1801, and consequently can ground my opinion only on the reports of others, yet from the great mortality that took place amongst them, during the first eight months after their having possession of the islands, I am

almost tempted positively to conclude that it was principally the pestilential fever to which their very great loss of men, amounting to more than a third of their complement, and as I am informed of more than half that were taken ill, must be attributed. This conclusion is contrary to the reports of the physicians who attended them; but I well know that one of the principal of those gentlemen was always averse to admit that the fever was contagious.

Dr. Scot, of St. Thomas, positively assured me that it was the pestilential fever which raged among the royals and other European troops stationed there. His description marked it accurately, and from the intercourse between the islands, it is more than probable that it was communicated to the sixty-fourth regiment in St. Croix, and occasioned principally the mortality amongst them. One circumstance, however, makes a difficulty against this conclusion. It is, that Dr. Steedman, a physician of the first reputation, who attended the troops at Frederickstadt, as consulting physician, on their first landing and

until the arrival of the staff physician from Martinique, declared to me that the fever among the British there was only the common remittent of the country, as it affects new comers, and that the mortality among them was to be attributed to erroneous medical practice, consisting in the abuse of evacuations, particularly antimonial emetics, in the delay of giving the bark, and in the bad quality of the latter medicine with which they were provided. Upon altering the practice, by being sparing of evacuations, by the early application of blisters, by mitigating the symptoms of the fever by the use of opiates and sudorifics, prudently administered, by the use of the cold-bath, and above all by giving bark of the best quality early and liberally, he diminished the mortality of the sick under his care to a very small proportion. But allowing that the fever which first attacked these troops was the common remittent, and that owing to erroneous practice or other circumstances, it was more fatal than usual, yet it does not account for the continuance, and even increase of that mortality during the healthy months, which are those

of May, June, July and August, when are mit-
tent fever was hardly to be found among the
other inhabitants of the island, and when the
troops were under care of the most eminent
practitioners who were accustomed to the treat-
ment of that disease, which usually under
their hands was seldom mortal, and when it
was so, proceeded generally from want of
timely application or neglect. It could not
be owing to the state of the atmosphere, for
that was favourable, at least to the other in-
habitants, nor could it be owing to fatigue,
exposure, or deficiency in necessaries to which
troops in a campaign are liable. The best
houses in the country were hired as barracks,
in proper situations; no duty required, but
what was necessary both for health and dis-
cipline; strictest regularity observed; the best
provisions, and liquor in proper quantity al-
lowed, and regularly distributed. The regi-
ment in St. Croix, and I believe those in St.
Thomas were as well clothed, fed, and kept
in as regular discipline, without fatigue, as
any in the service, and yet if I can trust re-
port the mortality amongst them amounted
to near one half their complement in ten

months, and likewise of the women who attended them; an extraordinary occurrence in the West Indies, the climate of which seems to be favourable to females. While their good conduct merited our applause, the care taken of them, and the liberal allowance afforded them excited our admiration. We could not help contrasting them with the very opposite condition of Danish soldiers and sailors before their arrival, for afterwards General Walterstorff, by command of the Crown Prince, made such alterations and improvements with regard to the former, as to put them, in every respect, on a footing equal to that of the British. But previous to this period, the condition of the Danish soldiery was highly unfavourable to their health.—They were crowded in lodgings under the batteries of the fortress, the guard was kept in a passage or entry, open at both ends, one of which faced the north, and as their uniform consisted intirely of linen, it was of little protection to them when the wind blew from that quarter.

A routine of thirty years had authorized a species of discipline by no means regular;

every man provided for himself, no messes were established, and their pay above sixpence per day, with the rations of bread and butter allowed, was disposed of for new rum, in which the greatest number indulged to excess.

Hence the appearance of the men during the greatest part of the year was feeble and unhealthy, especially in the fall; when the rains came on or the northerly winds blew, the half of them were then generally in the hospital, and almost all the rest in a convalescent state. Remittent fevers were then common, and when neglected very dangerous. Yet it has happened more than once that in the course of three months almost every individual in garrison has been under my care, and some of them frequently from repeated relapses, without the loss of a single man. The full complement of the garrison was 120 men; it rarely, however, exceeded 100, and the loss at an average was about ten per cent. annually, from June, 1781, until June, 1793, shortly after which period the pestilential fever appeared and increased the mortality.

The condition of the Danish sailors was even more unfavourable to their health, than that of the soldiers. They underwent the most severe labour in unloading their vessels, were exposed to excessive heat in the holds of them, or to that of the sun in transporting their cargo from the shore to the stores, either by carrying heavy burdens on their shoulders, or dragging them on trucks or low wheeled carts, like horses. In loading their vessels they were, if possible, more subjected to heat and fatigue ; would often, reeking hot from the hold, expose themselves, almost naked, to rain or a strong breeze of wind, drink large draughts of cold water, and qualify it, as they thought, by a large quantity of new rum mixed with it, or drank raw after the violent heat and fatigue of the day. Their general custom was to sleep on the decks exposed to the air, dews, and rain of the night, nor could any remonstrance or authority prevent them from this dangerous indulgence.

Yet upon the whole, to the best of my recollection, the proportionable number of

deaths among them in the above twelve years, did not exceed that of the garrison, except in the year 1785, when a violent hurricane upset or drove on shore every vessel in the harbour. The damage done to the houses in the town, and the extravagant rate of house-rent, obliged the crews to live in tents on shore, exposed to almost perpetual rain, and every morbid influence of climate, together with the fatigue and labour of repairing their vessels. Still the proportion of deaths was inferior to that of the least malignant species of contagion, though their condition, after its appearance and effects were known, was improved by the paternal solicitude of the Danish government. It was prohibited under severest penalties to employ them in carrying or dragging burdens on shore, and negroes were employed to assist and alleviate their labour on board. Nor did the deaths happen in a few days or weeks as in the case of infection, but in the course of seven or eight months, during which time the men were afflicted not only with fevers, but with pneumonic affections, dysenteries, and other abdominal complaints, and were exhausted by

frequent relapses from a continuance of existing causes. The hospital register books can prove my assertions with respect to them, and the same authority will confirm what I have said respecting the sailors. Limited as my experience has been, it has afforded me more than sufficient proof of the contagious nature of the fever, the fatal effects of which I have as above related. At the same time, it is with satisfaction I reflect, that by proper measures I have often prevented its attacks, and though numbers were destroyed yet many were rescued, and its progress in later years speedily and effectually checked.

Notwithstanding that Great Britain, in the late war, sustained losses by sickness far exceeding any former period in the West Indies, and that not only in the army, but with respect to every description of its inhabitants who have visited or settled in their colonies, yet no establishment was made in any place, to my knowledge, to prevent the communication of infection, or in other words the importation of death.

But Great Britain has not suffered alone; other nations have had their share of this fatal pestilence. The mortality at the Havannah, St. Domingo, and in some of the districts of the Spanish Main was such as to occasion quarantine establishments to be made there.

Two-thirds of the Dutch troops sent to take possession of St. Eustathius after the treaty of Amiens, perished in less than a twelve month after their arrival, though the island, being little more than a dry rock, is reckoned healthy.

The havoc among the powerful French army at St. Domingo is well known to have been occasioned by disease, and that among troops most of whom had served in Italy or Egypt, and accustomed to a warm climate.

Monsieur Villaret de Joyeuse, a brigadier-general, and brother to the commander in chief at Martinique, touched at St. Thomas, in his way, and there informed General Mullenfels, the present governor-general of the

Danish islands, that the French, in sixteen months, out of 28,000 men had lost two-thirds of the number, and of the remainder, not 6000 were capable of bearing arms.

To troops accustomed to the hardships of war, as they were, and to warm climates, no other cause could have occasioned so great and sudden a loss, but a malignant pestilence, and its existence amongst them is, I believe, not denied. Add to what is above related, the depopulation of flourishing cities in North America, of Cadiz, Malaga, and Gibraltar, and they will contribute to form an accumulation of evidence superior to any argument. Its irresistible force must establish the fact, that the pestilential fever which has destroyed so many thousands in the western world, and now threatens Europe with devastation, is of a highly contagious nature. The British government, though late, seemed now fully convinced of it, and proper precautions are now established by authority.

If the measures adopted are proportioned to the exigency of the occasion, and the ob-

servance of them properly enforced, I have no doubt but that Great Britain will be protected, or very partially affected, by this destructive scourge, and even its colonies liberated from its power.

But to accomplish this purpose all nations must combine. The Americans, the Danes, and the Spaniards have shewn laudable examples, and if the others concerned follow them with vigour, there will be every reason to hope that this pestilence will be effectually exterminated.

On my arrival in Denmark, in September, 1803, I was happy to find the government was thoroughly convinced of the infectious nature of the fever that had raged in their colony, and had taken precautions against its importation. Quarantine establishments were made all over the kingdom where necessary, particularly a very complete lazaretto at Christiansand, a port at the entrance of their dominions, situated between the western extremity of Norway and the Cattegat, on a line of coast filled with harbours, which are

much frequented by all the nations that navigate the Baltic.

Shortly after my arrival in Copenhagen I received a letter from the quarantine commission, in which they proposed eleven questions to me respecting the pestilential fever and required my answers. Much about the same time the government sent me an order to attend their meetings, and assist with my opinion and advice. A severe fit of illness prevented my personal attendance, but I gave them the result of my experience in my answers to their questions. Of these, not having a copy, I can only give you the substance from recollection. It was, that the pestilential fever did not arise from putridity of any animal or vegetable substance, nor from uncleanness and filth, heat, humidity, marsh miasmata, or any quality of the atmosphere; but was human contagion, most probably first generated by disease in human bodies, propagated and continued by effluvia from them or substances that had come within the reach of their influence. That cotton, coffee, or any substance in any state whatever, that

had not been impregnated by the above effluvia could neither generate or propagate the infection. That the usages of quarantine establishments against the plague, and those at Philadelphia against the above fever, with respect to the cargoes of vessels, appeared to me unnecessarily burdensome, tedious, and expensive.

That precautions against their importation ought to be carefully taken, and the means of stopping their progress when imported, strictly enforced.

But that these should be confined to the sick themselves, their apartments, clothes, bedding, or whatever had been in the same room with them, or approached their bodies.

That in an infected ship, apartment, or hospital, the means I have mentioned should be practised and every place and substance purified by washing, ventilation, &c.

That coffee, new goods in bales, barrels or

boxes, or any merchandize whatever, that had not been long exposed to the effluvia of diseased persons, by serving them as beds to lie upon, or confined with them in a close apartment, could never communicate infection, and might be easily purified by simple ventilation. But that clothes, rags, bedding of all kinds, should be purified by being well soaked in water and afterwards aired; or if suspected to be strongly impregnated with the miasma, to be immediately destroyed.

I complained of the obstacles that those concerned in trade threw in my way when executing my duty, particularly of the perverseness and negligence of the pilots, who thought themselves, perhaps, interested in thwarting my measures. But if regulations be established for preventing the importation of so destructive a pestilence, the observance of them in every point should be enforced by severe penalties, for indulgence and relaxation must render them useless. At the same time I was convinced from experience that every necessary measure might be taken with very little impediment to trade. Among

a number of facts to prove that putrefaction and filth did not give rise to the pestilence, I shall select the following. In the year 1781, a gentleman who managed the mercantile concerns of John Brown and Son, of Copenhagen, in St. Croix, received from them a cargo of provisions, which he, in expectation of higher prices, kept in his store-houses, until the stench from them incommoded the neighbourhood, and could be perceived in the street. A complaint was made to the master of police, who ordered the provisions, consisting of beef and pork, to be examined; and they were found in such a putrid state, that upwards of a thousand barrels were ordered instantly to be carried outside of the harbour, and sunk in the sea. No appearance of infection or of sickness of any kind appeared in the house, in the neighbourhood, or among the people employed in transporting the provisions. In the year 1786, I think, a Swedish brig came into the harbour of Christianstaëdt, in distress; she had suffered much at sea, was leaky, and in coming in struck on a reef at the entrance of the harbour, from being unmanageable. She was

got off with difficulty, and brought into the carenage to be hove down and repaired. Her cargo, which consisted of coffee in bulk, was in such a state as to be condemned as useless, and thrown on shore, where it rotted and formed a dunghill. This lay for several weeks near the carenage which was frequented by sailors and people from different nations; and yet, though it was in the hot months, no sickness or infection was occasioned by it. It was finally removed through my remonstrances*. About fifteen years ago a large whale, in a state of putrefaction, was driven on shore at the Estate Longford, in St. Croix. It was in summer, when the crop was over. The proprietor ordered large quantities of the blubber to be cut off, and boiled in his sugar coppers to supply his numerous concerns with oil, and for sale. The negroes of the adjoining estates, likewise supplied themselves with it. Yet none of them or of the white

* Let this important fact, which indeed is confirmed by what happens every year on coffee estates in the West Indies, be compared to the singular assignment of the pestilence of 1793, by Dr. Rush, to the effluvia of damaged coffee. How baseless the fabric of his theory! C.

inhabitants were affected with any sickness, though the stench from the whale could be perceived at a quarter of a mile distant. To these I may add this general fact, that Danish ships have seldom arrived at St. Croix from Denmark, without having their provisions in a greater or less putrid state; but sickness has never been occasioned thereby. To you, who have made this subject the study of your life, and whose success in preventing the ravages of the small-pox, at Chester and its neighbourhood, proves the truth of your opinions, and the utility of your publications, further details would be unnecessary. By happy discoveries, and by patriotic and benevolent exertions, among which your own hold the first rank, one dreadful pestilence, the small-pox, must be soon extinct from want of means of propagation. Another now claims your attention, and requires perseverance and ardour to correct the prejudices and false opinions that still, by several, are retained concerning it. To assist in correcting error by supporting with the irresistible evidence of facts, is the intention of this letter; and though it never would have been

written without your particular desire, yet I feel myself gratified in being able to add my feeble testimony of the necessity of adopting your important proposals for preventing and exterminating contagious diseases. Twelve years ago, before I had received accounts of the effects of the pestilential fever in Philadelphia, and very vague ones from Grenada, I warned the government in St. Croix, by memorials, and my fellow-citizens by advertisements in the newspapers, of the contagious nature of the pestilence that threatened them. Much travelling since in America and the West Indies, conversation with medical and other scientific men, and a continual addition of facts, have confirmed me in the opinion beyond the reach of doubt.

Had it been my good fortune to have met with your publications sooner, my observations might have been directed to more useful points. One in particular I might have been better able to ascertain: "whether clothes or such like could imbibe the infection by being merely brought into the patient's room or near his person, and convey it to a dis-

tance, so as to infect others?" Until lately I had but one opinion on the subject, which I thought confirmed by the experience of ages, and it was, that clothes could imbibe and convey infection, if they remained long in a close room with a patient or near his person. Therefore, in the beginning of the first pestilential epidemic, I regularly changed my clothes and washed myself after visiting patients in this disorder. But their increasing number, at one time nearly 100 daily, and calls being frequent and irregular, allowed me only to change my coat, which was of cloth, and the rest of my dress of cotton and linen. Even this precaution was afterwards laid aside, and I contented myself with only remaining a little time in the open air before I approached any one liable to receive infection, after having visited an infected patient.—Never, to my knowledge, or even suspicion, was the infection conveyed by me to any person. The facts adduced by you have recalled to my memory the foregoing observation, and I sincerely wish, for the benefit of mankind, that your opinions may be confirmed

by future experience*. But to enter into this subject further, and into others, which it suggests, would exceed the bounds of a letter already too long protracted.

I am, Sir, with esteem and consideration,
Your respectful Servant,
JOHN GORDON.

Bath, March 8th,
1805.

* The fact seems to be, that the chance of imbuing clothes with the infection of a pestilential disease, is always in proportion to the length of time during, and the distance from the infected person, at which, clothes, that is, wearing apparel, are exposed to it. Consequently the wearing apparel of the physician or medical man who visits the infected person, are by no means likely to propagate the infection. But it is not so with the wearing apparel of nurses and other attendants who are almost continually and necessarily within the infected atmosphere enveloping the sick; and certainly with them every possible precaution should be taken to destroy the infection with which they are impregnated, before they are used in the near vicinity of healthy persons. C.

APPENDIX, No. 2.

*Extract of a Letter from Dr. HAYGARTH
to the College of Physicians at Philadel-
phia, dated 16th October, 1806.*

“ I think proper to acquaint you, that in consequence of earnest exhortations, I have prevailed upon Dr. Fellowes, physician to the forces at Gibraltar, to undertake a diligent inquiry in what manner the pestilential fever was brought into that garrison. He is returned to England and has read his papers to me. After much patient and judicious investigation he discovered, beyond all doubt, that one *Sancho* had been an inmate of a family at Cadiz, when they were ill of this pestilence; that he left Cadiz on the 25th of August, 1804, and was attacked with this distemper in Gibraltar, whither he had returned, on the 27th. *Sancho* kept a retail grocer's shop. Near it was a canteen, or liquor shop, whither the fever soon spread.

From these two houses with which numerous visitors had undoubtedly hourly intercourse, who had not the least suspicion that they contained any infectious patients, many families in the near neighbourhood speedily received this mortal distemper. Dr. Fellowes shewed me a ground plan which he had taken of the streets first infected, that illustrated in an excellent manner the progress of the epidemic. In consequence of the fatal error and positive assurances that this pestilential fever was not infectious, you know that it rapidly spread, attacked about 12,000 and became mortal to between 5 and 6000 patients.

“Dr. Fellowes visited Cadiz, where he received a full confirmation of the above account of Sancho. He went to Malaga and discovered in what manner the pestilential fever had been imported into that city. In this journey he obtained authentic information from the chief magistrates and physicians, that the ravages of this pestilence were not confined to places connected with tides, as is said to be the case in America, but that it

was fatal to hundreds in several inland towns of Spain. Dr. Fellowes, will, I hope, soon publish his very valuable observations on this subject, which undoubtedly will be of inestimable importance to America.

“ Dr. Gordon, physician general of the Danish West Indies, now in Bath, is preparing for the press some decisive proofs of the infectious nature of the pestilential fever. During his residence at St. Croix, he witnessed its importation into that island at five distinct times. His testimony ought to convince your government of the great importance of a strict observance of the quarantine laws, in regard to ships from the West Indies. In a New York newspaper (New York Evening Post, December 27th, 1805,) I find that your health officer, at Philadelphia, complains, and undoubtedly with great reason, of the terrible mischief produced by the creed which still unfortunately prevails in America, that the pestilential fever is not infectious. But, what is still more astonishing, I find that the health officer himself,

who superintends the quarantine at New York, doubts whether it can be imported.

“ From numerous reports published by the anti-contagionists, I am clearly convinced that the sphere of infection in the pestilential fever, is confined to a very narrow distance, and that its progress might be effectually prevented by separation, cleanliness, and ventilation, exactly in the same manner as has been ever since 1783, successfully accomplished in regard to the typhus fever at Chester, and since 1796, at Manchester, and many other towns in Britain and Ireland.”

*College Square, Bristol,
26th March, 1808.*

My dear Sir,

Respecting your inquiries into the origin of the fever which devastated the garrison of Gibraltar in the year 1804, I can say nothing from my own experience as the disease had ceased to exist long before I reached that place in March, 1805. However I found on my arrival, from the univer-

sal report of all the survivors, learned and unlearned, and all the gentlemen of the profession, except one solitary exception, that the disease had been of a highly contagious nature, and that in almost every instance where circumstances had been duly observed, the progress of the disease from the sick to those subsequently attacked could be evidently traced. Of the malignancy of the disease there could be no doubt from its rapid progress and speedy termination in dissolution. And its ravages had been such among all classes as to leave a sentiment of horror on the minds of the survivors at the bare idea of its recurrence among them next summer.

When this malignant fever first made its appearance at Gibraltar, about August or September, 1804, it appeared that Dr. Nooth, the medical superintendant general, had given it as his opinion that the disease was not of a contagious nature. An opinion that most unhappily was attended with the most calamitous consequences, as from his learning, experience, and high professional rank, it had for a time its full weight among

all classes of persons at Gibraltar, and prevented those precautions from being adopted in the beginning which might have arrested the progress of the contagion.

But the alarming progress it made, the disease and death attending it, stalking, as it were, from door to door, soon opened the eyes of thinking men, and convinced them that Dr. Nooth's opinion was erroneous, but which he unhappily did not relinquish, but endeavoured to support by different modes of hypothetical reasoning, as I learned in some of my conversations on the subject with different gentlemen of the medical staff. However, in spite of this opinion, some families and individuals secluded themselves from all communication with those attacked with the disease, and if they had not, previous to their seclusion or segregation contracted the semina of infection, they experienced a happy exemption from this fever. Which was strikingly illustrated in the case of the family of Colonel Fyers, commanding engineer, a very intelligent gentleman, who with his numerous family secluded themselves in some retired

part of the rock, and held no communication with the rest of the garrison or inhabitants. They all escaped the contagion, and are living monuments of his discernment and discretion.

After the disease had abated, having exhausted itself by its own ravages partly, and the progress of infection being arrested by the approaching winter, as well as by the precautions that were ultimately adopted by authority, when the opinion of Dr. Nooth lost its influence; the attention of all the faculty, and the heads of departments, and indeed of every thinking person at Gibraltar, was diverted to the investigation of the origin of the disease. As Gibraltar had ever been considered as a healthy situation, and though it had been one hundred years in our possession had never before experienced such a calamity, and I hope, now that men's eyes are open, never will again.

For quicquid delirant *medici* plectuntur *ægroti*, as Horace says, of kings and their subjects.

Inquiries accordingly were instituted, by authority of the governor and his staff, and evidences collected, circumstances traced, and facts expiscated. And the result was, that the fever was introduced into Gibraltar by a person (whose name I forget)* from Cadiz, who left an infected house at that port, and fled to Gibraltar, where he put up at a low public-house, I believe called the Sol Ardent, where the disease appeared in his person, and was afterwards communicated to the rest of the inmates of that house, and from that house, as from a focus or centre, this malignant fever was traced to spread itself all around; every new nidus of the disease becoming a fresh point of emanation of its baneful influence till it had completed its ravages.

Being, from some new established regulations in the medical department of the navy, precluded from holding a medical function in the hospital department, notwithstanding my former professional service in the West Indies, I was sent to Gibralt-

* His name was Sancho. See Dr. Haygarth's Letter, p. 257.

tar, as agent of the naval hospital there; but had no medical charge. I was unpleasantly situated, and soon perceived that I was an object of jealousy to the medical folks of that department with whom I was associated, and in some measure under their controul. This, therefore, cramped my inquiries, and precluded me from investigating the subject as minutely as I could have wished; though most ungraciously circumstanced in that appointment, I confined myself to the duties of my office, if possible, to live in peace with those men. But my precautions in this respect availed me nothing: I was jostled out of my office, such as it was, and turned off like an old horse to graze on a common, without any half pay or provision for past services.

But to return:—The circumstances above mentioned respecting the importation of the fever into Gibraltar I learned at second-hand, in conversations with different intelligent persons, in particular Major Wright of the royal artillery, who had collected abundance of materials on the subject, I understood, for publication.

The situation of the *town* of Gibraltar, from its narrow streets and crowded houses at the foot of, and under the lee of, a huge mountain, when the Levant wind blows, is peculiarly calculated to foster the semina of contagion if once introduced there. And the circumstance of the town of St. Roche, only four miles distant, having not experienced this dreadful calamity, may be attributed not solely to the precaution of the inhabitants, who were guarded in their communications with infected places in the neighbourhood, but to its being constantly brushed, and the air of the place incessantly shifted, by the Levant wind, it lying opposite the Isthmus, and not to leeward of Gibraltar mountain.

Yours, most respectfully,

JAMES ROBERTSON, M.D.

Dr. Chisholm,

Clifton.

APPENDIX, No. 3.

Bath, March 2, 1808.

My dear Sir,

I have been favoured with your letter of the 29th ult. In answer I have to observe, that during the short time I was at New York, I had but little opportunity of making particular inquiry concerning the objects referred to. I can, however, assure you, that I found New York as I did all the towns I passed through, from Portland in the district of Maine to Fredericksburgh in Virginia, extremely clean; and if a comparison was made between them and the seaport towns in other parts of the world I have visited, certain I am, it would appear decidedly to their advantage.

I was at New York in November, 1805. The weather was then cold, and the epide-

mic, which had prevailed to a great degree in the months of September and October, was then rapidly on the decline. On that occasion I must say, I could not discover in the streets, docks, or slips any collection of filth, or impure source, from whence it might be supposed exhalations could be produced that would give rise to the prevalence of epidemic fever.

I remain, &c.

JOHN STEWART.

Dr. Chisholm,
Clifton.

APPENDIX, No. 4.

*Extract from the Log of the Ship Hankey,
from the time of her leaving the Island of
Bulama to her release from Quarantine,
noting all the deaths on board that Ship.*
(Beaver's African Memoranda, p. 471.)
See letter, p. 127.

1792.

Nov. 23. Sailed from Bulama.

24. Observed, lat. 11. 26. N. The west end of Bulama bearing N. by W. one half west 4 leagues, and the east end of Galenas N. N. W. three quarters, W. $3\frac{1}{2}$ leagues.

25. Died Mrs. Curwood, colonist; and George Wilkinson, seaman.

26. Anchored at Bissao; died Mr. Munden and Mrs. Hancorne, colonists, and John Mitchell, the ship's carpenter.

28. Died Edward Fowler.

1792.

Dec. 1. Died Charles Robinson, colonist.

3. Sailed from Bissao ; three of the crew taken ill of *the* fever.

4. Ship run a-ground.

5. In the afternoon sent a boat to Bissao to procure assistance ; the ship beat a great deal all night, but did not make any water.

6. Ship continues a-ground, and beats very hard.

7. Ship labours a great deal, but continues tight ; *all hands on board sick.*8. At noon the pinnace returned with a schooner and long boat, which carried out an anchor and cable ; at five got the ship off and made sail, but stuck fast again ; died Mr. Woody, the third mate, and boy Dick, the apprentice ; *all the people who came from Bissao in the pinnace taken ill.*

9. Got the ship afloat again ; died Patience Bates, and Joseph —, boy.

10. Charles Wood, seaman, died.

1792.

- Dec. 11. Mr. Birkhead, colonist, died.
12. John High, seaman, died.
13. Anchored again at Bissao; Mr. Rowe, colonist, died; his death is noticed as if he had not been on board the Hankey, but had died in the town of Bissao.
19. Richard Curwood, colonist, boy, died.
21. Sailed from Bissao with the assistance of some men, who left them in the evening.
26. At noon anchored in St. Francis's Bay, St. Jago, having mistaken it for Port Praya.
30. Mr. Gandell, colonist, died.

1793.

- Jan. 4. Anchored at Port Praya.
13. The Charon, Commodore Dodd, arrived; died Elizabeth Curwood, girl, colonist.
23. The Charon sailed, having sent two seamen to the Hankey.
24. The Scorpion arrived.
26. The Scorpion sailed, having sent two seamen to the Hankey.

1793.

Jan. 27. The Hankey sailed from Port Praya.

Feb. 4. *Died Samuel Hodge, seaman, one of those who came from the Charon.*

14. Arrived at Barbadoes.

15. Sailed.

16. Anchored at St. Vincent's.

17. Sailed.

19. *Anchored at Grenada.*

Mar. 27. William Mosely, seaman, deserted.

May 31. The carpenter drowned.

July 23. Received six men from the commodore, and sailed with the convoy from Grenada.

28. Anchored at St. Kitt's.

Aug. 1. Sailed.

Oct. 2. Anchored in the Downs, *and ordered to perform quarantine.*

5. Sailed for Stangate Creek.

8. Anchored at Stangate Creek.

18. *Delivered up the Bulama baggage.*

24. Released from quarantine.

29. Moored at Irongate.

THE END.

